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SUPPLEMENTS TO THE MONTHLY WEATHER REVIEW

During the summer of 1913 the issue of the system of publications of the Department of Agriculture was changed and simplified so as to eliminate numerous independent series of bureau bulletins. In accordance with this plan, among other changes, the series of quarto bulletins—letters from A to Z—and the octavo bulletins—numbered from 1 to 44—formerly issued by the U. S. Weather Bureau have come to their close.

Contributions to meteorology such as would have formed bulletins are authorized to appear hereafter as Supplements of the MONTHLY WEATHER REVIEW. (Memorandum from the Office of the Assistant Secretary, May 18, 1914.)

These supplements comprise those more voluminous studies which appear to form permanent contributions to the science of meteorology and of weather forecasting, as well as important communications relating to the other activities of the U. S. Weather Bureau. They appear at irregular intervals as occasion may demand, and contain approximately 100 pages of text, charts, and other illustrations.

Owing to necessary economies in printing, and for other reasons, the edition of SUPPLEMENTS is much smaller than that of the MONTHLY WEATHER REVIEW. SUPPLEMENTS will be sent free of charge to cooperating meteorological services and institutions and to individuals and organizations cooperating with the bureau in the researches which form the subject of the respective supplements. Additional copies of this SUPPLEMENT may be obtained from the Superintendent of Documents, Washington, D. C., to whom remittances should be made.

The price of this Supplement is 20 cents.

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PREFACE

Every season, since time immemorial, has illustrated the individual farmer's dependence on weather. Society as a whole shares in this dependence, for widespread droughts have caused famine, great migrations, and other economic and social change, especially where reserves of the means of existence were already slight and where society was incapable of meeting the consequences of extensive crop failure.

It was demonstrated in the great droughts of 1934 and 1936 in the United States, that calamity can be averted through economic and social organization. But these droughts also emphasized anew that scientific research thus far has failed to discover those natural laws that may underlie the recurrence of drought, and to formulate those principles by which the time and the extent of drought and other great changes in weather might be anticipated. It is well recognized, however, that such laws and principles underlie many of the most fundamental problems of agricultural prosperity and social welfare, in the immediate as well as in the more distant future. Hence men, motivated by scientific curiosity and by the desire to find a better basis for social action, have endeavored to find means in accumulated meteorological records and other data, for forecasting weather beyond the few days ahead.

The papers in this volume represent part of a research project financed by funds appropriated by the Bankhead-Jones Act of 1935 to the Department of Agriculture "to

conduct research into laws and principles underlying basic problems of agriculture in its broadest aspects." It is believed that contributions of the kind represented here will further the knowledge of laws and principles underlying weather changes which present problems that are basic to agriculture and the public welfare.

The cooperation of meteorologists in the Weather Bureau and at Massachusetts Institute of Technology and Harvard University has been most helpful in the preparation of these reports. The research was organized and supervised by a committee consisting of Dr. C. H. Rossby of Massachusetts Institute of Technology, Mr. L. H. Bean of the Office of the Secretary, Mr. L. F. Page and Dr. C. F. Sarle of the Bureau of Agricultural Economics. An editorial committee, consisting of Dr. E. W. Woolard of the Weather Bureau, Dr. H. C. Willett of Massachusetts Institute of Technology, and Mr. L. F. Page of the Bureau of Agricultural Economics, has reviewed the manuscripts. Acknowledgment is also due others in the Weather Bureau and other institutions who have offered helpful suggestions. However, the authors of the separate papers must be considered solely responsible for their conclusions.

INTRODUCTION

BY

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The status of knowledge about long-range weather forecasting has always been somewhat difficult to determine. At one extreme there are those who claim that the problem has been almost or completely solved. Some of these are charlatans, but most of them are misled by their desire to believe in the systems they have developed and by their willingness and that of their friends to give their forecasts the benefit of any doubt in verification. At the other extreme are those who have seen claim after claim disproved, system after system fail under actual forecasting conditions. They are convinced that there will never be successful long-range forecasting, and that time and effort spent on this field are wasted. They are as anxious to disbelieve in any system as others may be to believe in it.

These subjective attitudes make evaluation of methods difficult. But it is necessary to know what has been done—whether the results are positive or negative—in order to take advantage of the years of work which have been spent on this problem. The papers published here are the result of an attempt to get an objective appraisal of the value of certain approaches to long-range weather forecasting.

The search for periodicities, the method of attack that has been most widely employed, has not been considered here, except in one case. To do this would require a duplication and, in many cases, an extension of the original work. Furthermore, no method of determining the existence and significance of periodicities is universally accepted. A complete solution has not been obtained for problems of change of phase, serial correlation, and freedom of the form of the periodic curve. Sufficient attention has not been given to the avoidance of errors of both kinds as defined by Dr. J. Neyman—errors of acceptance of a false hypothesis and errors of rejection of a true hypothesis.

In the class of periodicities are the many attempts to find a relation between sunspot cycles and weather phenomena and, of course, studies involving astronomical motions. Even where the field is thus limited, the clerical work necessary to test all of the many claims that have been made would be tremendous.

The appraisal was therefore mainly restricted to those theories that include a meteorological hypothesis—some physical explanation that appeared to be at least rational. In many cases these explanations had been arrived at a posteriori, hence the conclusions were not so impressive as if an a priori hypothesis had been substantiated by the data available.

In several cases analyses of methods have been supplemented by verification of forecasts or of statistical formulas. It should be pointed out that this is not a conclusive test of the theory. The verification series can be considered only as another sample of data, and the significance of the result must be compared with the size of the sample. In the verification, however, we have the equivalent of a test of an hypothesis; consequently a sample of a given size is more effective than in the case where the data are used to form the hypothesis. There is, further, a practical consideration, especially in the case of negative verification. If public confidence would be lost by a failure of forecasts during a period as long as the sample verification period, then whatever the statistical significance of the test, its practical implications should be considered.

REPORT ON THE WORK OF G. T. WALKER

By R. B. Montgomery

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I. INTRODUCTION

Sir Gilbert Walker, born 1868, at first taught mathematics, was Director General of Observatories in India 1904–24, Professor of Meteorology at the Imperial College of Science and Technology 1924–34 (in this position he succeeded Sir Napier Shaw, and on retirement was followed by D. Brunt), and is now Editor of the Quarterly Journal of the Royal Meteorological Society.

Walker's studies fall into several groups. Rather than present them in chronological order I will start with his "world weather" studies, which are of more importance to us than the other groups. Next will follow a brief chapter on the climatology of India with mention of Africa and South America, taken largely from Julius Hann's "Handbuch der Klimatologie," preparatory to the two concluding chapters on Walker's contributions to seasonal forecasting. His work in statistical theory is omitted, but references to it are included in the bibliography.

At the end is given a bibliography of Walker's publications. While it is admittedly incomplete, it includes papers on all phases of Walker's work.

II. WORLD WEATHER STUDIES

In his early work on forecasting the monsoon rainfall in India by correlation methods Walker soon noticed that data from a large area tended to behave uniformly, both within the area and in relation to the surrounding areas. This led him to the conclusion that, even for forecasting local conditions, it was necessary to study conditions over the whole globe in a statistical manner. This he proceeded to do.

1. *Sunspots.*—He first prepared three papers correlating sunspots with rainfall, temperature, pressure (**29, 30, 31**), respectively. The first of these he introduces with a discussion of the evidence for a positive relationship between sunspot numbers and the solar constant, which he seems to consider quite favorable, but not conclusive. He uses for the correlations the annual meteorological elements at a number of stations over the world and the mean sunspot number for the contemporary year. The number of years of data varies greatly with the stations, but for most stations it is over 30. The correlations for each element are given in tabular form and are also plotted on a world chart.

In the paper "Sunspots and Rainfall" he uses the data from 152 stations and also the flood water in the Nile, the outflow in the Mississippi, the volume of the Caspian Sea, the level of Lake Constance. In conclusion he says:

it would appear that the coefficient * * * is not in general larger than would be produced by mere chance. * * * It is only where the coefficients over a region have some appreciable tendency toward uniformity that a real relationship may be concluded. The relationships seem real in the case of the Nile (0.16 for 1749–1800 and 1825–1903, 0.24 for 1865–1912) and India (for individual stations the coefficients are 0.2 or less); but perhaps the clearest case is South America, where below latitude 30° rainfall is deficient where sunspots are numerous.

(Santiago —0.20, 1853–1911; Cordoba —0.07, 26 years; Buenos Aires —0.20, 1861–1907; Pelotas —0.36, 1893–1907; Azo —0.19, 1858–1912; Punta Arenas —0.43, 1888–1907). The pattern of positive and negative areas on the chart is too complicated to be stated in a few words. He emphasizes that the study should have been applied to homogeneous regions rather than to individual stations, but convenient data for regions were not available; for temperature and pressure individual stations are more representative of the surrounding regions. It occurs to me that there is considerable possibility for bias in the correlation coefficients as a result of the necessary checking and subsequent correction or omission of early unreliable data, as indicated by the two values above for the Nile flood and by his tabulation of data at Cordoba for 1873–1907 although he uses only 26 of these years.

In the paper "Sunspots and Temperature" he uses 97 stations. The resulting chart shows negative areas over most of the world, the only positive ones being the South Pacific Ocean (represented only by Auckland 0.27, 51 years), part of the Indian Ocean (Seychelles 0.10, 1894–1913; Carnarvon 0.07 for 19 years), the Gulf of Mexico and Bermuda (Galveston 0.10, 1873–1912; Key West 0.11, 1873–1912; Bermuda 0.17, 35 years), and a broken area extending from northern Africa through Spain, France, and the British Isles to northern Siberia. The regions of highest negative correlation are India and southern China (—0.15 to —0.45), southeast Australia (—0.30 to —0.50),

Santiago (−0.38, 1861–1913) and Cordoba (−0.33, 1873–1913), and all of North America with the exception of Galveston (−0.05 to −0.35).

He says:

* * * it may be pointed out that the paradox in question (prevailing occurrence of lower temperature with increased sunspots) would be explained if the increase in wind due to increased radiation were to bring sufficient increase of humidity, cloud and rain over land areas to send down the temperature at ground level, although the temperature would presumably be higher than usual in the upper air. If this interpretation be correct, other tropical or semitropical places in which we might look for a rise, or at any rate a diminished fall, of temperature would be in large desert regions to which damp air could not penetrate, and possibly in islands over which the air might be expected to be so nearly saturated at all times that no increase of humidity would have any marked effect on the temperature. The only station above 10,000 feet in the tropics with a long series of reliable data known to me is Leh (36 years) and there the correlation coefficient of +0.01 is in marked contrast with those of Agra and Calcutta, −0.43 and −0.44, respectively. In relatively dry regions Denver (40 years) has a coefficient of −0.03, Alice Springs (35 years) one of −0.04 and Algiers (24 years) one of +0.13; while of small islands in the tropics, Seychelles (20 years) has +0.10 and Port Louis, Mauritius (38 years) has −0.07, though St. Helena (16 years) has −0.17 and Honolulu (38 years) has −0.20.

A feature to which attention has not hitherto been drawn, I believe, is the tendency in the polar regions of the Eurasian continent towards higher temperatures at times of sunspot maxima. In the Southern Hemisphere the only station tabulated south of latitude 50° is Punta Arenas (20 years), with a coefficient −0.02; and no long series of data is here available for any station south of 54°. But to the north of 63° there are seven stations of which particulars are given in the following table:

Station	Number of years	Correlation coefficient	Latitude
			° ′
Archangelsk	39	−0.08	64 33
Christiansund	37	+.20	63 7
Obdorsk	25	+.16	66 31
Turuchansk	22	+.24	65 55
Vardö	39	−.03	70 22
Jacobshavn	39	−.16	69 13
Stykkisholm	63	−.03	65 5

In the paper "Sunspots and Pressure" he uses 91 stations. His summary is:

It is a striking fact that the region of negative coefficients extends from India over a considerable area, including northern Africa, east and south Africa, Arabia, Persia, Java, and Australia. Europe, except in its most southern districts, Siberia, the China coast and Japan appear to have positive coefficients. As might be expected from the opposition in pressure between the Argentine or Chile and India the coefficients in the former countries are strongly positive; and in the east coast districts of North America positive coefficients prevail, though in the west there is a tendency towards negative values. In the Pacific (e. g., Honolulu and Wellington) the relationship appears to be generally positive, but in the Atlantic there is a negative region including Iceland, Scotland, and the North Sea.

A comparison of the chart with the corresponding chart in a previous paper for sunspots and rainfall will bring out the general tendency for the pressure coefficients to be opposite in sign to rainfall coefficients; and it may be inferred that the variations of pressure and rainfall are to a large extent dominated by the same cause, being to a comparatively small extent affected by variations of temperature. * * *

Walker states that according to Brückner one would expect increased solar radiation as indicated by sunspots to cause an increase in the general circulation and hence an accentuation of the average pressure distribution. He finds partial verification for this: negative sunspot-pressure correlation for Stykkisholm (−0.04, 63 years) and positive for Ponta Delgada (0.21, 1894–1912) and Honolulu (0.25, 1883–1912), but negative values in the equatorial region are found only in the Indian Ocean. The stations within 10° of the Equator are Zanzibar (−0.46, 1892–1913); Seychelles (0, 1895–1913); Colombo (−0.38,

1870–1913); Batavia (−0.27, 1866–1910.) Superimposed on this effect seems to be a tendency for negative coefficients in the Eastern Hemisphere and positive ones in the Western, which is in accord with his later concept of the southern oscillation.

Unfortunately there are large regions of the earth for which Walker has no stations, for instance none south of 63° S. in the Eastern Hemisphere and, except for rainfall, none south of Honolulu between Auckland and Punta Arenas. He repeatedly mentions another handicap, that he has used only annual values. In chapter I of "World Weather I" (36) he gives some seasonal and monthly correlations, which however do not appreciably improve the cause of sunspots. Of interest is the following table of correlations between monthly Indian temperature and sunspots for 1875–1906:

January	−0.25	July	−0.37
February	−0.32	August	−0.22
March	−0.43	September	−0.38
April	−0.17	October	−0.36
May	−0.11	November	−0.29
June	−0.50	December	−0.31

It will be noticed that in India the negative values are greatly reduced during the hot weather. In the concluding chapter IV of the same paper he states his opinion that variations of world-weather tend to occur in a definite manner, i. e., to be associated with definite swayings or surges of pressure, and that changes in solar conditions tend to favour or check these weather changes; such oscillations if they had been strictly periodic might have been regarded as forced oscillations of a type corresponding to the natural weather oscillations of the atmosphere.

Walker's conclusion that sunspot numbers play a definite but very minor direct role in yearly or seasonal weather seems to me the correct one. But he by no means exhausts the possibilities of the sunspot approach. It may be, as some authors believe, that the total sunspot number is not the measure most closely connected with the weather. Again, it may be that the time rate of change of sunspot numbers is the important factor. This seems especially likely if the physical connection is through changes in total solar radiation. For we know that deviations in total solar radiation from normal rarely amount to more than 1 percent, while seasonal weather deviations amount to much more, so that solar radiation could effect the changes in weather only through a disturbance of equilibrium. Again, if Walker's picture of a natural oscillation of the atmosphere receiving impulses via solar radiation is correct, the effect of an impulse in increasing or decreasing the amplitude will depend entirely on the phase of the oscillation at the time the impulse arrives.

Walker also attempts to make use of Abbot's solar radiation values in his correlations, but since he had data for only 15 years it does not seem worth while to discuss his results.

Nothing further need be said concerning his studies relating to sunspots, for he does not later arrive at new conclusions in this respect. In fact, he later states (51):

So we are led to the view that the southern oscillation merely expresses a natural oscillation or system of surges in the general circulation. * * * If this is granted we suppose that an increase in the number of sunspots or of solar radiation will increase slightly the general circulation and so bring about the observed relationships with sunspot numbers.

Sunspots are briefly discussed in "World Weather III" (50) and in "World Weather IV" (59), confirming earlier statements. In "World Weather V" (61) Walker further states:

The relationship of an increase in sunspots with a decrease in the general circulation in the North Atlantic and North Pacific Oceans

is noteworthy, and warns us against overconfident inferences regarding solar activity.

In a brief note (68) he offers objections to C. E. P. Brooks' physical explanation, and to the reality, of the correlation of 0.87 between sunspot numbers and the level of Lake Victoria, 1896–1921, and extends the data to 1935.

2. *Early papers on the relationship between seasonal weather of different regions.*—We now go on to discuss the material which is contained in I, II, III, V of his papers entitled "World Weather." ("World Weather IV" will be discussed in the chapter on forecasting.)

Chapter II of the first of these (36) gives the intercorrelations of 17 centers of action, 15 being pressure centers and the other 2 being India Peninsula (June to September) and Java (October to February) rain. A table for each center gives its correlations in the two quarters, December to February and June to August, with all other centers two quarters before, contemporary, two quarters after. Each table is accompanied by a brief discussion; there are small additional tables of rainfall and snowfall correlations, and under "General remarks" he says:

Perhaps the most striking feature of the pressure correlations is the cooperation of the group Azores, Charleston, Honolulu (June to August), Samoa, South America (June to August), and Peninsula rain on the one hand, and of the group Iceland, Central Siberia, Northwest India, Port Darwin, Mauritius, and southeast Australia on the other; members of the groups have positive contemporary correlation coefficients with each other and negative with those of the other group. We can perhaps best sum up the situation by saying that there is a swaying of pressure on a big scale backwards and forwards between the Pacific Ocean and the Indian Ocean, and there are swayings, on a much smaller scale, between the Azores and Iceland, and between the areas of high and low pressure in the North Pacific. * * *

Chapter III, "Temperature variations," gives tables for a number of localities of temperature correlated with contemporary and previous pressure at the same place, and other tables. Walker finds no temperatures having high correlation with subsequent weather,[1] so he concludes that "it is pressure which controls temperature." In regard to the Benguela Current flowing northward near the southern west coast of Africa, he finds the small coefficients for 17 years between Cape Town temperature December to February with St. Helena temperature of the same quarter of 0.04, one quarter later of 0.26, two quarters later of 0.18. He also discusses conditions in the South Orkneys, but has data for at most 14 years.

In chapter IV, "Physical interpretation of the relationships," Walker discusses, among other things, Hildebrandsson's view that "the types of season were propagated from west to east like waves." Since for 11 years the correlation between June to August South Orkneys temperature and Punta Galena (Chile, 40° S.) pressure is 0.45, he concluded that the eastward current would be warmer and would carry lower pressure when South America pressure is high. This would explain the supposed eastward propagation of a reversed wave in about 6 months as indicated by the coefficients:

South America pressure	With Cape Town pressure, December to February
3 quarters before Cape Town	−0.38
2 quarters before Cape Town	−.48
1 quarter before Cape Town	−.29
0 quarter before Cape Town	.12
1 quarter after Cape Town	−.42
2 quarters after Cape Town	−.16

The South America values are the mean of Buenos Aires, Cordoba, and Santiago; the data are for 1875–1921;

[1] In a letter commenting on this report, Walker writes, "The conclusion quoted, that there are 'no temperatures having high correlations with subsequent weather,' has certainly been negatived by subsequent work."

the isolated −0.42 he considers accidental. The correlations between Cape Town and Mauritius are very small. "It seems that the antarctic influence arrives directly at Mauritius, not across Cape Town." The coefficients between southeast Australia (Brisbane, Adelaide, Alice Springs) pressure June to August and Mauritius pressure −2, −1, 0, 1, 2 quarters later are −0.02, 0.30, 0.43, 0.04, 0.17 (1876–1921); "the movement here also appears to occur eastwards and to take 1 or 2 months." The relations in the Pacific are even less clear. June to August pressure in South America has a coefficient of 0.48 with Samoa pressure two quarters later, and southeast Australia and Darwin December to February respectively have coefficients of −0.41 and −0.62 with Samoa one quarter earlier (all for 1890–1910), which indicate a westward propagation of some sort from South America. Of course, it is impossible to determine whether these waves move along the middle latitudes where the stations are located or by the shorter route through the antarctic.

The coefficients tabulated in "World Weather II" (39) extend and largely replace those of "World Weather I." St. Helena and South Orkneys were dropped, and the pressures at Vienna, San Francisco, Tokyo, Cairo, and temperature at Dutch Harbor were added. Tables for each center for each of the four quarters give its correlations with all others −2, −1, 0, 1, 2 quarters later. Each table is accompanied by a brief discussion of the reality of the coefficients, based largely on the statistical tests. Thus the first table shows that Iceland pressure (Stykkisholm) December to February has six contemporary coefficients greater than the probable largest, 0.31, for the 16 centers with records of 36 years or more. These are central Siberia (Irkutsk and Eniseisk) 0.34, Vienna −0.52, Azores (Ponta Delgada) −0.54, Charleston −0.32, Tokyo −0.38, Cairo −0.42. "There are 3 exceeding 0.4 and the probability of this owing to pure chance is only 0.0003; also the probability that chance would produce the two exceeding 0.5 is only 11 in 100,000."

3. *The three oscillations.*—In "World Weather II" Walker first gives names to his three oscillations, and in chapter III discusses the two northern ones.

North Atlantic oscillation.—To the mechanism of the oscillations in the North Atlantic considerable attention has been devoted by Pettersson, Meinardus, Hildebrandsson, Helland-Hansen, Nansen, and others, and it is generally recognized that an accentuated pressure difference between the Azores and Iceland in autumn and winter is associated with a strong circulation of winds in the Atlantic, a strong Gulf Stream, high temperatures in winter and spring in Scandinavia and the east coast of the United States, and with lower temperatures in the east coast of Canada and the west of Greenland.

The correlation coefficient of Iceland (Stykkisholm) winter pressure with that of Vardo (1875–1920) accordingly proves to be +0.44, and with contemporary Vardo temperature −0.64; with contemporary winter temperature in the eastern coast of the United States (Charleston + Washington) the coefficient is −0.42, that of the following spring, −0.18 and of summer, +0.16. The coefficient of Iceland pressure, September to February, with contemporary temperature at Vardo is −0.58, and with the quantity of ice at Newfoundland next spring and summer, as given by Meinardus, is −0.72. * * *

The statement regarding the Gulf Stream is later repeated (51). They are at present debated questions as to whether the southwest winds appreciably affect the Gulf Stream, as to whether the strength of the Stream appreciably affects ocean temperatures, and as to whether the latter appreciably affects European air temperatures. Thus G. C. Simpson ((61), Discussion) "did not think that the high temperatures in Scandinavia were due to the warm sea water brought by southwest winds. The relatively high temperature in Scandinavia was due to an increased frequency of southwest winds and had little to do with the temperature of the sea water."

In discussing the influence of ice, Walker gives a favorable review of Wiese's work. Walker finds coefficients of 0.28 and −0.46 between the amount of ice in the Greenland Sea (April to July) and pressures in the subsequent autumn at Vardö and Vienna. Wiese showed "that in summer and autumn an excess of ice drives the cyclonic tracks in Europe southward and affects the temperature and rainfall."

At this point it is well to insert mention of a later paper (45) in which Walker reviews Wiese's work and finds that some of the latter's basic coefficients do not persist. Thus in regard to Wiese's coefficient of −0.44 between South Orkneys temperature and Barents Sea ice for 10 years, Walker finds for June to August 1903–23 only 0.02 (coefficients tabulated in (50)). In general Walker's studies indicate no connection between the Arctic and the Antarctic. Walker further notes that the correlations of 0.25, 0.44, 0.31, between Grimsey temperature March to May and the following December to February temperatures at Berlin, Kristiansund, Vardö 1882–3 to 1918–9 (except 1896), which are similar to Wiese's, are reduced to 0.0, 0.08, 0.10 by including 1880 and 1881, "the latter being a phenomenally cold year at Grimsey." "It would be unwise to accept without further examination Wiese's conclusions that variations in Barents ice and of rain in the equatorial regions are determined by the same variations in the general circulation." "It would appear that conditions in the Barents Sea are of importance in a fairly large region of the Northern Hemisphere: and their usefulness is greatly enhanced by their persistence. My calculations show a correlation coefficient of 0.84 (0.86 1895–1925 given in (50)) of June to August ice with that of April and May of the same year, of 0.60 with that of June to August ice of the previous year, and of 0.44 with Greenland ice of the following year."

North Pacific oscillation.—This is similar to that of the North Atlantic, according to Walker, consisting essentially of an opposition between pressures in the Aleutian LOW and in the North Pacific HIGH as represented by Alaska and Honolulu respectively. Unfortunately there is no station near the center of the HIGH, Honolulu being on the western edge in winter and on the southern edge in summer. Likewise there was no series of pressure data near the center of the Aleutian LOW, the Alaska group being made up of broken series from Juneau, Atlin, Sitka. Dutch Harbor temperature, however, has a close correlation with pressure in the region because subnormal pressure accompanies abnormal northerly winds in the rear of the LOW, lowering the temperatures. The coefficients with Alaska pressure are: December to February, 0.68; March to May, 0.48; June to August, 0.12; September to November, 0 (18–19 years); the low values in summer and fall are without significance because the LOW fills up in summer.

In regard to North American conditions associated with this oscillation he states that we should "expect opposition between winter temperatures at Dutch Harbor and at Victoria, British Columbia on the eastern side of the depression: for the years 1892–1919 in which some Esquimalt data are included the coefficient is −0.06, but for 1903–19 it is −0.40."

In order to verify the interpretation placed on the data of the North Pacific I have verified in terms of them the conclusions reached by Henry regarding rainfall in the North Pacific Coast States. I find a coefficient of −0.54 between that rainfall and the December to February pressure at Alaska, while with Dutch Harbor for this period the relationship is −0.24; with Alaska pressure in autumn the relationship is −0.20. I have also verified that as indicated by Stupart there is a relation between the winter pressure at Alaska and the winter temperature at Winnipeg, the coefficient being −0.54.

He discusses in considerable detail the work of Okada on Japanese conditions.

This oscillation differs from the North Atlantic oscillation because, for one thing, ice is here an unimportant factor. Walker quotes Krümmel as saying that "Behring Straits form no exit gate for polar ice formations, and heavy ice is limited to the northerly portion of the Behring Sea."

Southern oscillation.—In introducing chapter IV Walker states: "By the southern oscillation is implied the tendency of pressure at stations in the Pacific (San Francisco, Tokyo, Honolulu, Samoa, and South America), and of rainfall in India and Java (presumably also in Australia and Abyssinia) to increase, while pressure in the region of the Indian Ocean (Cairo, northwest India, Port Darwin, Mauritius, southeast Australia, and the cape) decreases." Most of the large coefficients for pressure in South America (the three stations "lie approximately on the axis of the high pressure belt") occur in the quarter J–A, there being six with contemporary, and nine with subsequent, conditions elsewhere which are greater than "the probable largest." In this "important winter period there are no stations where previous conditions control South America." "The natural inference is that South America in its winter is either one of the original controls," or is connected directly with one of the original controls." This leads to an interesting discussion involving Antarctic weather and ice conditions.

Along the coast line easterly winds prevail which set up a westward current, and a glance at a modern map of the Antarctic Continent will show that any ice borne by this current will, on reaching Graham's Land, be thrown right off in a northerly direction and be carried off toward the east by the easterly current which flows round the world in the latitudes 40° to 60°; a certain amount of ice may at times be thrown off at 170° E. by the western boundary of the Ross Sea, but the shape of the coast is not nearly so well adapted for the purpose as that at 60° W.: * * * * * * The view that Graham's Land throws off many of the icebergs is supported by an examination of their most northward limit, which is further north (35° S.) in the vicinity of the cape than elsewhere in the southern seas; and this feature also characterizes pack ice, though probably not to so great an extent.

The outstanding feature of the pressure distribution of the Antarctic is that, as in the Northern Hemisphere there are areas of low pressure in the North Atlantic and North Pacific, so in the southern region we have them in the Ross Sea, the Weddell Sea and the Bellingshausen Sea (to the east and west of Graham's Land); and just as the Artic ice is carried round Greenland and along Labrador into the Atlantic near Newfoundland, but finds no outlet through the Behring Straits, so the Antarctic is thrown off at Graham's Land into the southern Atlantic, but apparently not in any continuous stream elsewhere, such escape into the eastward current as occurs at other parts of the coast line being probably spasmodic and caused by local disturbances. Also just as in the North Atlantic there is a pressure opposition between the Azores and Iceland, the strengthening of which increases the ice flow of the Labrador current, so in the south Atlantic, as Mossman's important studies have made clear, there is an opposition between the high pressure belt across Chile and the Argentine on the one hand, and the low pressure areas of the Weddell Sea and the Bellingshausen Sea on the other.

When discussing the climate of Chile in 1911 Mossman said:

If the high-pressure belt is far south, as in the winters of 1903, 1908, 1909, and 1910, then there is a marked decrease in the winter rains all along the littoral between latitudes 30° and 45° S., but if the Graham's Land lobe of the Antarctic anticyclone extends northwards, as in the winters of 1902 and 1904, then the theatre of cyclonic activity also spreads northward, bringing heavy rains, strong north and northwest winds, much cloud and high temperature between latitudes 30° and 45° S. With these conditions winds at the southern extremity of the coast are light with a marked high, southerly component, temperature is low, pressure relatively high, and rainfall much under the seasonal normal.

"In such years as 1903, 1908–10, when the Pacific anticyclone is far south, a very steep gradient for NW. winds is set up south of about lat. 45°. These winds blowing with gale force for weeks together bring much rainfall and foul weather over an area stretching from north of Evangelists Islands to the South Shetlands."

Walker further finds that "in general a southward movement of the storm track is associated with or produced by an accentuation of the high pressure conditions in South America."

He derived certain results from pressure and ice data from the Antarctic, but the data are so sparse that it does not seem worth while to reproduce the discussion here.

It might be mentioned here that the rudiments of the southern oscillation were first discovered by H. H. Hildebrandsson and later confirmed by Norman Lockyer and W. J. S. Lockyer (51).

4. *Further correlations of quarterly pressure and temperature, etc.*—"World Weather III" (50) consists of 8 pages of discussion and 27 pages of tables of correlation coefficients. Table V provides an extension of the tables in "World Weather II." The relationships for Samoa pressure are recomputed with 36 years data available, Zanzibar pressure is added, and Wellington pressure is substituted for southeast Australia. The following temperature centers are added: North America (Winnipeg, St. Louis, St. Paul), Siberia (Surgut, Irkutsk, Tomsk), Honolulu, Batavia, St. Helena, Mauritius, Samoa, Cape Town, South Orkneys. Further additions are the Nile flood and the ice in Barents Sea (in April and May and June to August, 1895–1925). For each of the above-named centers a table shows the correlations with the other new centers as well as with the old ones, the arrangement being exactly as in "World Weather II." The new correlations with South America are referred to the mean of Cordoba and Santiago only. The table for South Orkneys temperature (1903–23) includes some coefficients with conditions elsewhere three and four quarters later. The total number of centers now available for study is 32. Table V and the ones in "World Weather II" are summarized in his table I which gives for each center and for each quarter the number of coefficients with preceding, contemporary, and succeeding conditions which are greater than the probable largest, as well as the total for each center. His table VI gives for each center and each quarter the correlation with sunspots of the contemporary quarter. Unfortunately he does not explain how tables II and III were computed, but the subject is covered later in "World Weather V."

The largest total numbers of coefficients, greater than the probable largest, listed in table I are: Darwin pressure, 87; Batavia temperature, 78; Samoa temperature, 70; northwest India pressure (Lahore, Karachi), 54; Zanzibar pressure has 27, and Wellington pressure only 4, and South Orkneys temperature only 2, each out of a possible 581. Nile flood has 31 out of a possible 145, and Barents Sea ice only 7 out of 289. While these figures give a rough indication of the extent to which conditions at the various centers depend on world weather rather than just local conditions, it should be remembered that the centers are not evenly distributed over the earth. Thus for Darwin merely local conditions would be expected to give high correlations with Batavia temperature (which accounts for 12 of the 87) and perhaps with northwest India pressure (another 12).

The numbers of coefficients listed in table I for centers in or near the United States are Iceland pressure, 17; Alaska pressure, 8; Charleston pressure, 22; San Francisco pressure, 24; Honolulu pressure, 31; Dutch Harbor temperature, 5; North America temperature, 27; Honolulu

temperature, 12. Of these the following may be of particular interest.

Alaska pressure December to February has coefficients of 0.64 (21 years) with Cape Town pressure of the previous quarter; 0.62 (18 years) with Dutch Harbor temperature of the previous quarter; and 0.66 (22 years) with India monsoon rainfall the previous summer. In the quarter September to November it has −0.54 (23 years) with itself two quarters before and −0.56 (19 years) with Dutch Harbor temperature two quarters before.

Charleston pressure December to February has −0.36 (1876–1915) with central Siberia pressure two quarters before; −0.42 (1875–1919) with northwest India pressure the previous quarter, and −0.50 (1882–1919) with Darwin pressure the previous quarter. In March to May it has 0.44 (1875–1921) with San Francisco pressure the previous quarter; 0.36 (1883–1920) with Tokyo pressure the previous quarter, and −0.44 (1875–1919) with Mauritius pressure the previous quarter. In June to August it has 0.38 (1875–1919) with Azores pressure the previous quarter; 0.52 (1875–1921) with itself the previous quarter; 0.40 with San Francisco pressure two quarters before, and 0.36 with Tokyo pressure two quarters before.

San Francisco pressure December to February has 0.44 (1875–1921) with itself the previous quarter and −0.42 (1876–1921) with southeast Australia pressure the previous quarter. In March to May it has 0.48 (1875–1921) with Charleston pressure two quarters before; 0.38 and 0.36 with itself one and two quarters before; −0.50 and −0.48 (1875–1921) with northwest India pressure one and two quarters before; −0.40 with southeast Australia pressure the previous quarter, and −0.36 (1875–1921) with Cape Town pressure two quarters before. In June to August it has −0.46 (1876–1915) with central Siberia pressure two quarters before; 0.42 with Charleston pressure the previous quarter; 0.44 with itself the previous quarter, and −0.38 (36 years) with Mauritius pressure the previous quarter. In September to November it has 0.36 with itself the previous quarter; 0.36 and 0.40 (1883–1921) with Honolulu pressure one and two quarters before; −0.38 with Mauritius pressure two quarters before, and −0.40 with southeast Australia pressure two quarters before.

Dutch Harbor temperature December to February has 0.36 (36 years) with Cape Town pressure the previous quarter. In March to May it has 0.36 (36 years) with Darwin pressure the previous quarter, and 0.42 (36 years) with itself the previous quarter.

North America temperature December to February has 0.40 (50 years) with Mauritius pressure two quarters before; 0.54 (51 years) with Batavia temperature the previous quarter; −0.46 (50 years) with India Peninsula rain the previous summer, and −0.40 (52 years) with the Nile flood the previous summer. In September to November it has 0.44 (52 years) with Cairo pressure two quarters before; 0.46 and 0.42 with Batavia temperature one and two quarters before; 0.50 (1890–1925) with Samoa temperature two quarters before, and −0.40 with the Nile flood the previous summer. The temperature of North America "has moderately close relationships in autumn and winter; in fact, it has moderately close relationships with all three oscillations. In general, temperatures are 'passive' rather than 'active,' and are suitable rather for being forecasted than for forecasting conditions elsewhere; but Batavia and Samoa have a good number of relations with subsequent weather."

Regarding the physical explanation of the southern oscillation, there has been little confirmation of the suggestion made in "World

Weather, I." (p. 126), that Antarctic conditions might play an important part through the quantity of ice flowing in the southern winter past the South Orkneys, and, in particular, that winters of abundant ice would be followed by low temperature and high pressure at the cape next summer. The coefficient of winter temperature at the South Orkneys with next summer's pressure at the cape is only −0.30, and with temperature there at that time only +0.24, the latter becoming 0.34 in the following autumn, and 0.56 in the succeeding winter. While, therefore, there is some indication of a sea-current from the South Orkneys towards the cape, it does not appear to exercise any marked control over pressure there, or over the southern oscillation as a whole. Most control over subsequent quarterly conditions is exercised by autumn pressure in northwest India, winter and spring pressure at Port Darwin, winter pressure in South America, the monsoon rainfall of the India Peninsula and the Nile floods; and this is more suggestive of a general increase of circulation than of control from any one region.

In discussing the North Atlantic oscillation Walker says:

Attention has been drawn by Wiese to the importance of ice in the Barents Sea, and data have been compiled for April and May, as well as for June to August, at which time the ice is disappearing. Ice in the former period has, with Azores winter pressure, a coefficient of −0.76, while the closest quarterly relationship between pressures at Iceland and the Azores is −0.60 in spring.

The rest of the discussion is largely in confirmation of his previous results.

In a short paper (57), Walker mentions a coefficient of 0.84 between June to August temperature at New Year Island and June to August temperature the following year at Cape Town for 1902-27. Of all Walker's correlations between two conditions widely separated in time, this is the only one for which a definite physical explanation can be offered. "The explanation probably is that during the winter months the ocean temperature controls the air temperature, and the water flowing eastward between Cape Horn and the Antarctic takes about a year to reach the Cape of Good Hope." Later (64), Walker correctly states that this close correlation is possible while those between Cape Town and South Orkneys are small, because conditions at these islands are quite effectively prevented by the Antarctic Convergence from affecting surface temperature north of it.

The bewildering wealth of correlation coefficients presented in "World Weather II" and "World Weather III" is summarized, and also extended, by a new and comprehensible presentation in "World Weather V" (61). Walker accomplishes this by first giving numerical definitions to the three oscillations, then the data from various stations are correlated with the three oscillations. Thus the number of stations can be extended indefinitely, for each station is correlated with only one or possibly two or all three oscillations (with various time differences), instead of with all other stations.

5. *Numerical definitions of the two northern oscillations.*—Walker has defined the North Atlantic and North Pacific oscillations for the quarter December to February only, and the southern oscillation for the quarters June to August and December to February only. The definitions are arbitrary. For instance, the well-known opposition in pressure between Iceland and Azores (the coefficients are December to February −0.54, March to May −0.60, June to August −0.48, September to November −0.40; 1875-1921) might lead to a definition of the North Atlantic oscillation as the difference in pressure departures at these two stations. But in order to obtain a more representative expression of the Azores HIGH it is well to include Bermuda and Vienna, and likewise Ivigtut for the Iceland LOW; it then turns out that Azores has a small correlation with the resulting oscillation, so it is dropped and other factors are added.

The final expression for the North Atlantic oscillation December to February is:

(Vienna pressure) + (Bodö temperature) + (Stornoway temperature) + 0.7 (Bermuda pressure) − (Stykkisholm pressure) − (Ivigtut pressure) − 0.7 (Godthaab temperature) + 0.7 (Hatteras + Washington temperature)/2.

The quantities in parentheses are departures from normal in units such that in each case the standard deviation is $\sqrt{20}$. Walker tabulated in the same units the oscillation for each year 1875–1930, and the stations involved in the formula for the years available. The correlations of these stations with the oscillation are:

Vienna pressure	0.76	1875–1930.
Stornoway temperature	.84	1875–1919, 1921–30.
Bodö temperature	.86	1875–1930.
Stykkisholm pressure	−.80	1875–1930.
Ivigtut pressure	−.84	1879–1918, 1920–26.
Bermuda pressure	.72	1875–86, 1888–1929.
½ (Hatteras + Washington temperature).	.72	1875–1930.
Godthaab temperature	−.70	1875–84, 1886–1928.

Of course if we were dealing with entirely random figures the sum of four series with the same standard deviations would have a coefficient with each of the component series which would average 0.5; but with eight series the average will be 8−1/2 or 0.35, and the above figures are much too large to be explained in that way.

The correlations of other stations with the North Atlantic oscillation are not tabulated, but are shown on figure 1, where coefficients based on less than 30 years are distinguished by brackets; and relationships with the rainfall of regions (of which the individual stations are given in Walker's table X) are enclosed in circles.

In the temperature chart it will be seen that in addition to the well-known warming in northwest Europe and cooling in Labrador, with coefficients as big as +0.86 and −0.70, there is a warming in the southeast of the United States with coefficients up to 0.70, and a cooling in the region to the southeast from Trinidad to Iraq, the coefficient of the Cape Verde Island being −0.48 and of Cairo −0.60. It is a natural conjecture that the warming in the United States of America may be due to a more northerly path of the LOWs and a more southerly direction in the winds, the cooling of the Cape Verde Islands to a more northerly direction in the winds, and the lower temperatures from Egypt to Iraq to a more southerly track of winter rain-bearing depressions. The chart of rainfall naturally shows an area of positive coefficients round Iceland, where the pressure coefficient was negative, and an area of negative coefficients over southeast Europe where the pressure coefficients are positive; but the correspondence of the rainfall and pressure coefficients is quite rough.

Thus the North Atlantic oscillation definitely represents an increase in the general circulation over the ocean. The most pronounced increase in pressure gradient is in the westerlies, but there is also an increase in the trade wind region; the increase in wind may be of the same magnitude in the two regions. Superimposed on this intensification is a northward displacement of both the Azores HIGH and the Iceland LOW, and a northward displacement of the storm tracks.

Unfortunately the few coefficients of the oscillation with previous and subsequent conditions given in his table II are very small. The largest are with St. Vincent temperature in the following March to May (42 years) −0.52, and with Newfoundland ice in the following March to August 1875–1912, 0.64 (March to July 1900–26, 0.42).

In regard to the North Pacific oscillation Walker says it was known—

that pressure variations at Hawaii were opposed to those over Alaska and Alberta, and that high pressure in Alaska meant a more southerly track of the lows and more rain in parts of the United States and liability to cold weather east of the Rocky Mountains. Since then perhaps the chief result of statistical examination has been the unexpected association of low pressure in the northern region with low temperature in the Aleutian Islands (Dutch Har-

bor), and it now appears that the association with high temperature in south-west Canada is even more marked.

The formula finally adopted for the N. P. O. [North Pacific oscillation] is—

(Honolulu pressure) + ⅓ (Qu'Appelle + Calgary + Prince Albert temperature) − ⅔((Sitka, Fort Simpson, or Juneau pressure) + (Dawson pressure) + (Nome pressure)) − (Dutch Harbor temperature).

Here the fractions ⅔ and ⅓ have been used because the quantities multiplied by them relate to places not far enough apart to be independent.

effects, there being positive coefficients in Canada and eastern China, and negative in eastern Siberia and the south of the United States. In table III are to be found some representative relationships with the conditions in the succeeding spring and with the previous November.

The charts for this oscillation bear further scrutiny than Walker has given them, and his implied conclusion, from

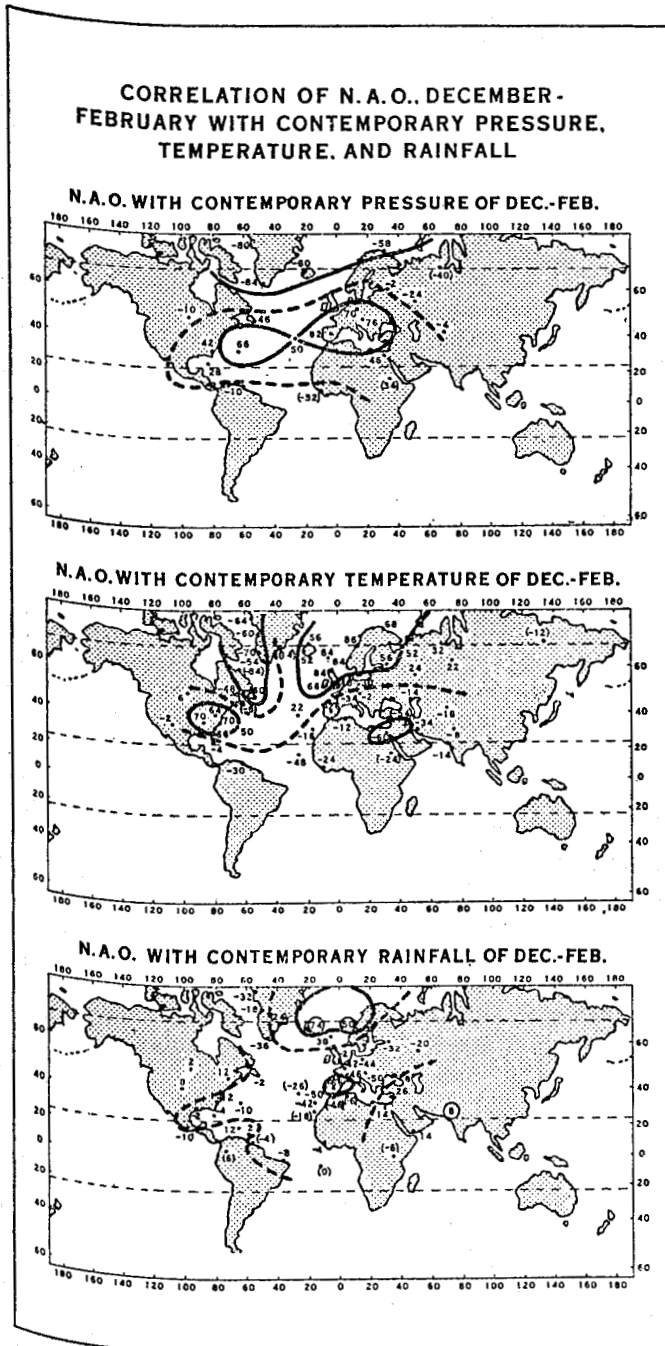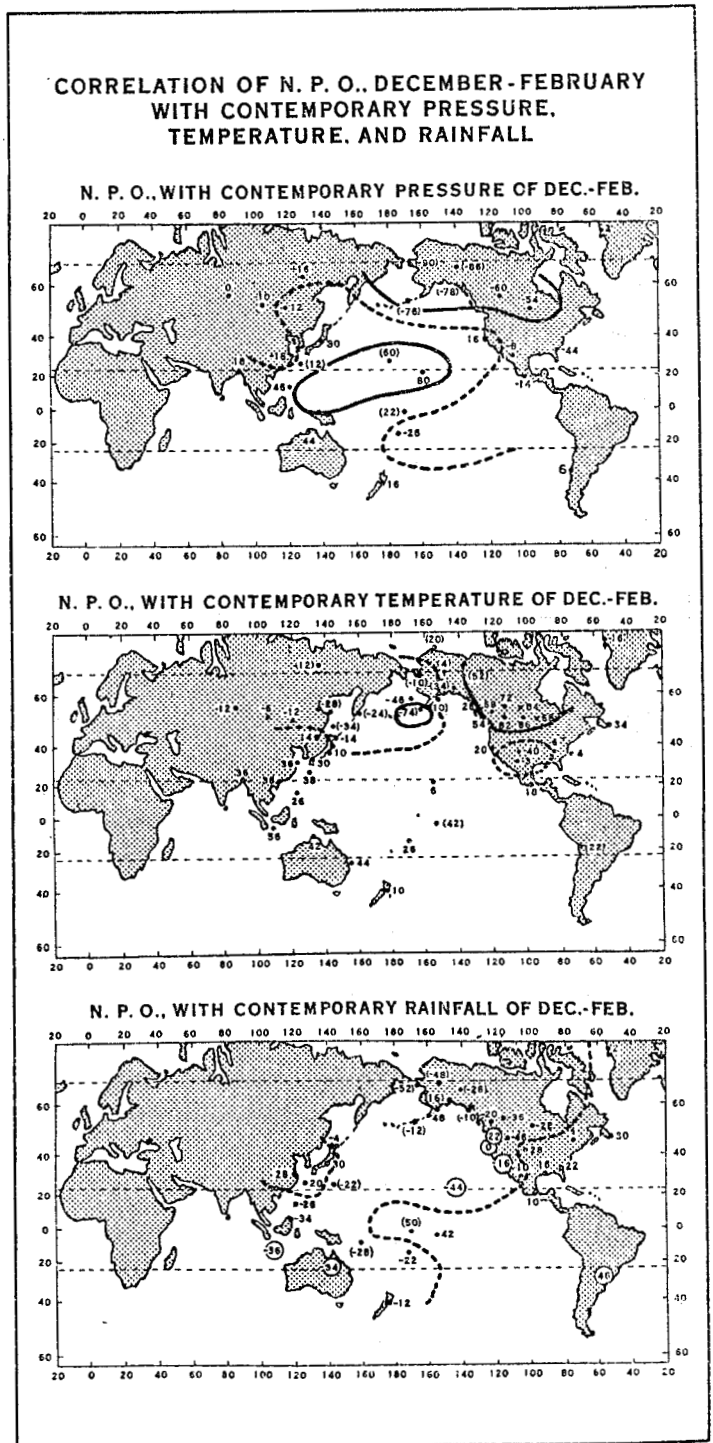FIGURE 1.—Reproduced from Walker and Bliss (61).

FIGURE 2.—Reproduced from Walker and Bliss (61).

The coefficients of the four factors with the North Pacific oscillation thus defined are respectively 0.80, 0.86, −0.86, and −0.74. The coefficients with the North Pacific oscillation of pressure, temperature, and rainfall over a wider area are given in [figure 2.] A comparison of this with [figure 1] shows that the features of the North Pacific oscillation occur decidedly farther south than those of the North Atlantic oscillation; but in spite of the wide difference in the conditions there is a marked similarity in the temperature

the similarity with the North Atlantic oscillation, that this one involves a general increase in circulation is not well proved. A comparison of figure 2 with a chart of the normal January pressure distribution (see fig. 9) definitely shows that the area of negative coefficients does not coincide with the Aleutian LOW, and that the positive

8

area does not coincide with the North Pacific HIGH; in fact the nodal line passes through the center of the LOW, and the eastern edge of the strongly positive area passes through the center of the HIGH. Furthermore, temperature at Dutch Harbor, which is normally at the center of the LOW, certainly indicates a displacement rather than an intensification of the LOW. Thus the North Pacific

CORRELATION OF S. O., JUNE-AUGUST
WITH CONTEMPORARY PRESSURE,
TEMPERATURE, AND RAINFALL

S. O. OF JUNE-AUG. WITH CONTEMPORARY PRESSURE

S. O. OF JUNE-AUG. WITH CONTEMPORARY TEMPERATURE

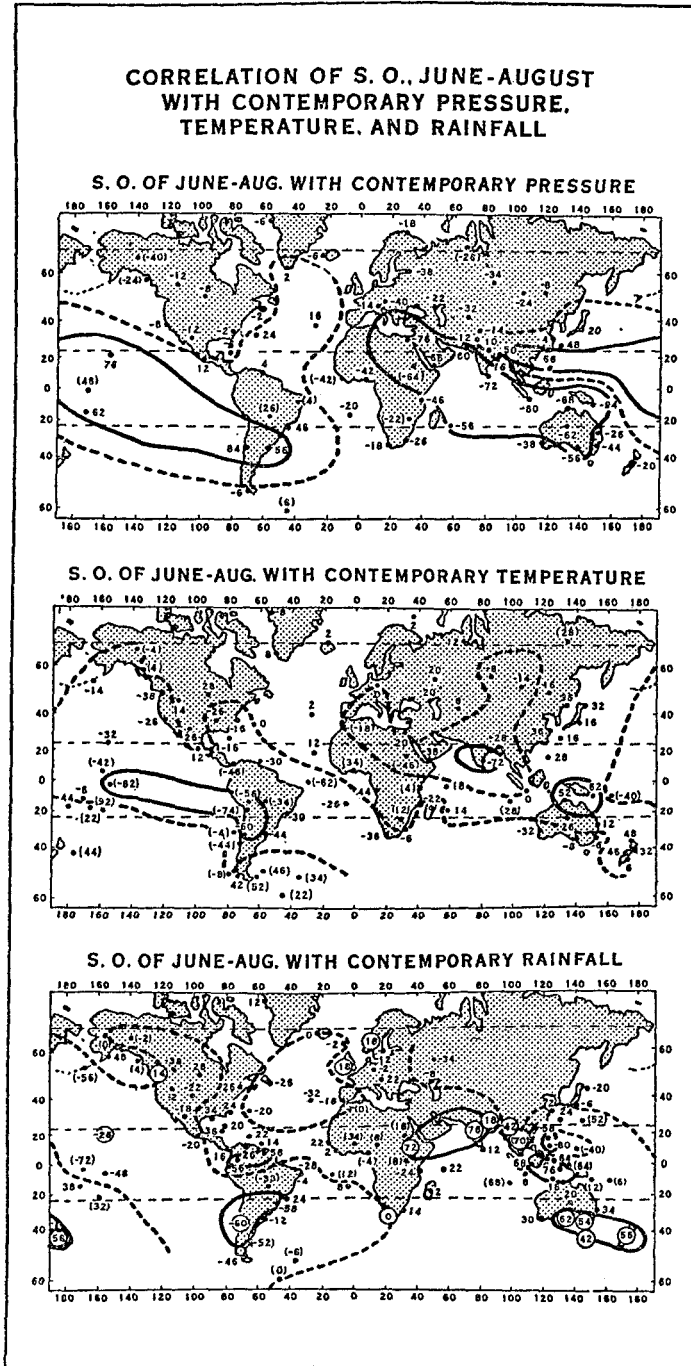S. O. OF JUNE-AUG. WITH CONTEMPORARY RAINFALL

FIGURE 3.—Reproduced from Walker and Bliss (61).

oscillation represents a westward displacement of the HIGH and a northeastward displacement of the LOW, which does not necessarily involve an increase in circulation. As regards the northeast trades, the chart indicates that they are increased by the oscillation in the eastern Pacific, but decreased in the western. The HIGH of western Canada is probably best explained as a result of stagna-

tion, so its decrease with the oscillation is perhaps an argument in favor of the latter being associated with increased circulation.

6. *Numerical definitions of the southern oscillation.*—In regard to the southern oscillation Walker says:

In general terms, when pressure is high in the Pacific Ocean it tends to be low in the Indian Ocean from Africa to Australia: these conditions are associated with low temperature in both these areas, and rainfall varies in the opposite direction to pressure. Conditions are related differently in winter and summer, and it is therefore necessary to examine separately the seasons December to February and June to August.

For the southern winter J–A the formula for the southern oscillation is:

(Santiago pressure) + (Honolulu pressure) + (India rain) + (Nile flood) + 0.7 (Manila pressure) — (Batavia pressure) — (Cairo pressure) — (Madras temperature) — 0.7 (Darwin pressure) — 0.7 (Chile rain).

"India rain" stands for the Peninsula and northwest India. "Chile rain" is the mean of 9 stations between 30° S. and 42° S. The contemporary correlations with the southern oscillation June to August are shown in figure 3. Its values are tabulated for 1875–1930, and plotted in figure 6.

Earlier (36) Walker remarks that "a general increase of circulation would send up those pressures which are normally above the general level and down those which are normally below it; and with this we should expect to find the raising of pressure over the biggest sea area (the Pacific) and the lowering of it over the biggest land area (eastern Europe, Asia, and much of Africa); * * *" This very general criterion for an increased circulation, of an exchange between the land and water hemispheres, is to some extent confirmed for the southern oscillation June to August. But the details of figure 3 bear little relation to the general circulation. There is a negligible tendency for the negative area to be concentrated in the equatorial and subpolar zones, and for the positive area to be concentrated in the subtropical zones, as an increased planetary circulation would demand. Nor does the chart indicate an increased monsoon circulation, for both Australia and South Africa are in the negative area.

It is best to conclude that the southern oscillation involves opposite departures of pressure in the Indian Ocean and in the Pacific, for which the cause is as yet unknown except that a pressure departure in any region must be accompanied by an opposite departure in another region, but which seems definitely established. Besides Walker's suggestion of a generally increased circulation with the oscillation, the only explanation he has offered is the propagation of waves from South America or the Antarctic (36), for which the evidence is very inadequate. Associated with the oscillation are a number of details which, though unexplained, are of great importance because of their regular repetition with the oscillation. The first is the westward displacement of the Asia LOW (figure 3 shows the nodal line passing through the center of the LOW) and hence of the India monsoon, resulting in more rain in India and Abyssinia, and less in Burma. Walker never mentions this obvious displacement of the monsoon, but considers excess rain in India, etc., as due to a greater intensity of the monsoon as a whole. Another detail shown by figure 3 is the decrease of the winter monsoon of Australia (and of South Africa to a less extent). This combines with the westward shift of the Asia LOW to decrease the monsoon system extending from Australia to China; the rainfall of this region will be discussed in the next chapter.

The formula for the southern oscillation December to

February involves the following centers; the "A centers" are given unit weight and the "B centers" a weight of 0.7. The sign with which each enters the formula is indicated in the last column, which gives its correlation with the resulting oscillation. The contemporary correlations with the southern oscillation December to February

CORRELATION OF S. O., DECEMBER-FEBRUARY WITH CONTEMPORARY PRESSURE, TEMPERATURE AND RAINFALL

S. O. OF DEC.-FEB. WITH CONTEMPORARY PRESSURE

S. O. OF DEC.-FEB. WITH CONTEMPORARY TEMPERATURE
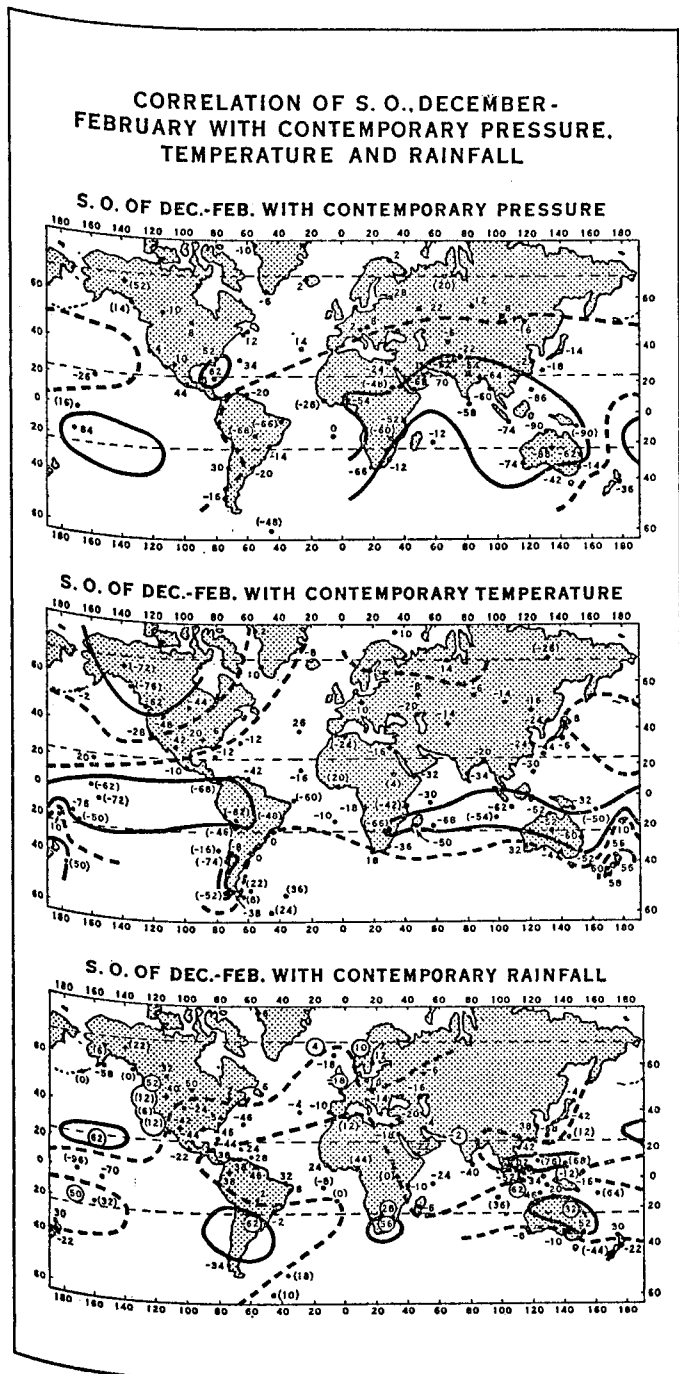
S. O. OF DEC.-FEB. WITH CONTEMPORARY RAINFALL

FIGURE 4.—Reproduced from Walker and Bliss (61).

ruary are shown in figure 4. Its yearly values are tabulated for 1882–1930, and plotted in figure 5.

This oscillation in December to February is essentially the same as in June to August but the positive and negative pressure areas are somewhat shifted. South Africa, as well as Australia, is now strongly negative, and South America is neutral instead of strongly positive, so the change in these two continents follows the change of the local monsoon tendency. The westward extension of the

positive area has retreated, so that Manila is in the strong negative area, although the South Pacific HIGH is displaced decidedly westward.

A centers		
Samoa pressure	1891–1929	0. 84
Darwin pressure	1882–1930	—. 90
Manila pressure	1887–1928	—. 86
Batavia pressure	1882–1930	—. 74
Southwest Canada temperature (Calgary, Edmonton, Prince Albert, Qu'Appelle, Winnipeg)	1885–1929	—. 74
Samoa temperature	1890–1930	—. 76
Northeast Australia rain (Derby and Halls Creek in Western Australia, 7 stations in North Australia, 20 throughout Queensland)	1882–1929	. 82

B centers		
Charleston pressure	1882–1930	. 52
New Zealand temperature (Wellington, Dunedin)	1882–1930	. 60
Java rain	1882–1930	. 62
Hawaii rain (12 stations)	1886–1930	. 62
South Africa rain (15 stations, Johannesburg the most northern)	1882–1929	. 56
Northwest India pressure (Lahore, Karachi)	1882–1930	—. 68
Cape Town pressure	1882–1930	—. 66
Batavia temperature	1882–1930	—. 62
Brisbane temperature	1887–1930	—. 60
Mauritius temperature	1887–1930	—. 60
South America rain (Rio de Janeiro and 2 stations south of it in Brazil; 3 in Paraguay, Montevideo; 15 in Argentina, of which Bahia Blanca is the southernmost)	1882–1930	—. 62

VARIATIONS OF THE SOUTHERN OSCILLATION, 1875–1930

WINTER (JUNE-AUG.)
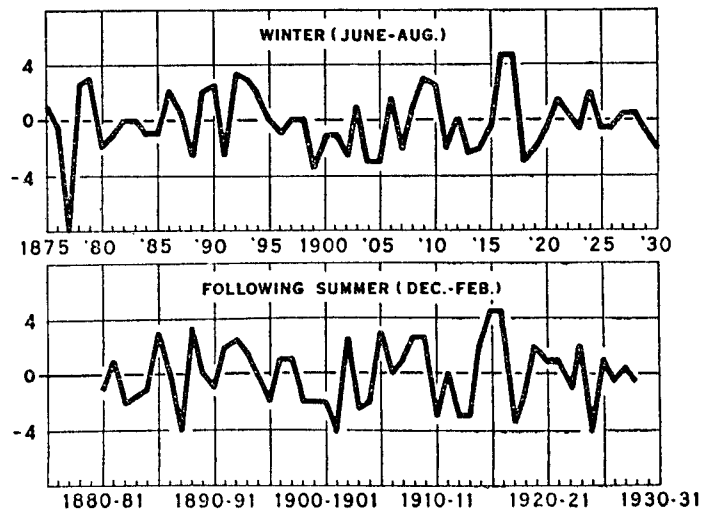
FOLLOWING SUMMER (DEC.-FEB.)

FIGURE 5.—Reproduced from Walker and Bliss (61).

It is difficult to find any suggestion of an explanation for a close connection of temperature and pressure in North America with the southern oscillation, so it seems to me a mistake to include these, as well as Hawaii rain, in the definition of the oscillation.[2]

The positive entry of New Zealand temperature in the formula for December to February is worth mentioning, because the temperatures of Brisbane and Samoa nearby

[1] In commenting on this report, Walker writes, "I think that the relationships of world weather are so complex that our only chance of explaining them is to accumulate the facts empirically: we know now that it was impossible to explain cyclones (LOWS) until data of the upper air conditions were available, and there is a strong presumption that when we have data of the pressure and temperature at 10 and 20 km. we shall find a number of new relations that are of vital importance. I do not think that anyone would have guessed that while Darwin pressure of June to August has a coefficient of −0.68 with the contemporary southern oscillation, Manila has one of +0.66; but such are the facts, and I see no justification for refusing to use them. The empirical evidence of the relationships of North American temperature and Hawaii rain seems to me satisfactory and I regard myself as logically bound to include them as factors in the oscillation. It may be remarked that, as experience shows, omitting them would make no appreciable difference to the series expressing the variations in the oscillation."

series are desirable. In the Indian Ocean, however, the first four squares, to the south of Ceylon, are in fair agreement and show that the southern oscillation in the relatively inactive December to February period produces a marked negative effect on sea temperatures lasting for half a year, just as it does on air temperatures at Mauritius and Manila. * * *

In the Pacific the first square, enclosing Honolulu, agrees with [figure 3] in having a negative coefficient and with [figure 4] in having a positive one. For the negative coefficients in the centre and east of the pacific down to latitude 30°, shown in [figures 3 and 4] we are reminded of the cold Humboldt current and of the relatively cool southeast and east-southeast winds blowing from over it, so that an increased circulation might furnish the explanation.

The concluding lines of table VIII confirm the previous impression that, although a strengthening of the southern oscillation produced in winter a decrease in the number of icebergs at the South Orkneys and an increase in the number of icebergs in the South Pacific, these effects are not big enough to justify a theory that the physical origin of the southern oscillation lies in the Antarctic. The association of large numbers of icebergs with abundant rainfall in India had previously been noticed.

After discussing the pronounced persistence of the southern oscillation from June to August to December to February, Walker says:

But if we look through tables IV, VI, VIII, IX for all the coefficients numerically exceeding 0.40 giving a half yearly foreshadowing of the southern oscillation of June to August, ignoring those based on fewer than 30 years of data, we only find the pressures of India (Calcutta, −0.48; Allahabad, −0.44; Rangoon, −0.50) and the rainfall of Southern Rhodesia (−0.46). For the season March to May immediately preceding the southern oscillation some additional relationships with pressure and temperature are given in tables V and VII. The corresponding factors exceeding 0.40 are the pressures of Batavia (−0.54), Samoa (+0.58), Santiago (+0.48), the temperature at Dutch Harbor (−0.42), and in May the Himalayan snowfall (−0.46) and the height of the Ganges (−0.50).

8. *The secular variation.*—Of the centers used in the formulae above, the secular change has been eliminated from those whose coefficients with time are greater than 0.1. This is done by taking departures not from the mean, but from the straight line giving best fit to the data plotted against a time scale. These reduced departures are used both in computing the oscillations and in correlating these centers with them. Walker nowhere states that the secular change has been eliminated from the other centers, so we must assume that it has not.

Walker's two reasons for eliminating the secular changes are "that there often is little cause for believing in their reality," and that they are "of little interest in the present investigation." In light of Scherhag's investigation (1936, Annalen der H. und M. M., p. 397) showing a continuous increase of the prevailing westerlies in the Northern Hemisphere during the last 50 years or more, there can be small doubt of the reality of the secular changes since Walker's agree with those found by Scherhag. By eliminating the secular change Walker has eliminated the larger part of the change of general circulation in the Northern Hemisphere, though the resulting oscillations may give a better picture of the year-to-year changes. Walker's oscillations do not agree closely with Scherhag's secular change in circulation. In particular Walker finds no correlation between the North Atlantic and North Pacific oscillations, while Scherhag finds an equal increase in circulation in both oceans. One solution of the discrepancy is that the secular change and the North Atlantic oscillation both represent an increased circulation, while the North Pacific oscillation represents a displacement with no increase of the circulation.

9. *Further remarks on world weather.*—Walker's summary to "World Weather V" is:

In order to form more definite ideas regarding the oscillations named, the North Atlantic, the North Pacific, and the southern series of figures have been derived to express the variations of each, and from these have been obtained their relations with pressure,

temperature, and rainfall over wide regions as well as the relations of the three oscillations with each other and with sunspots.

The southern oscillation in the southern winter is found to be extremely persistent, and its departure has a correlation coefficient of 0.84 with that of the following summer, thus providing a basis for foreshadowing seasonal conditions. The effects of Antarctic

FIGURE 7.—Reproduced from Walker (64).

conditions and of ocean temperatures are considered, but a satisfactory physical basis for the oscillation has still to be found.
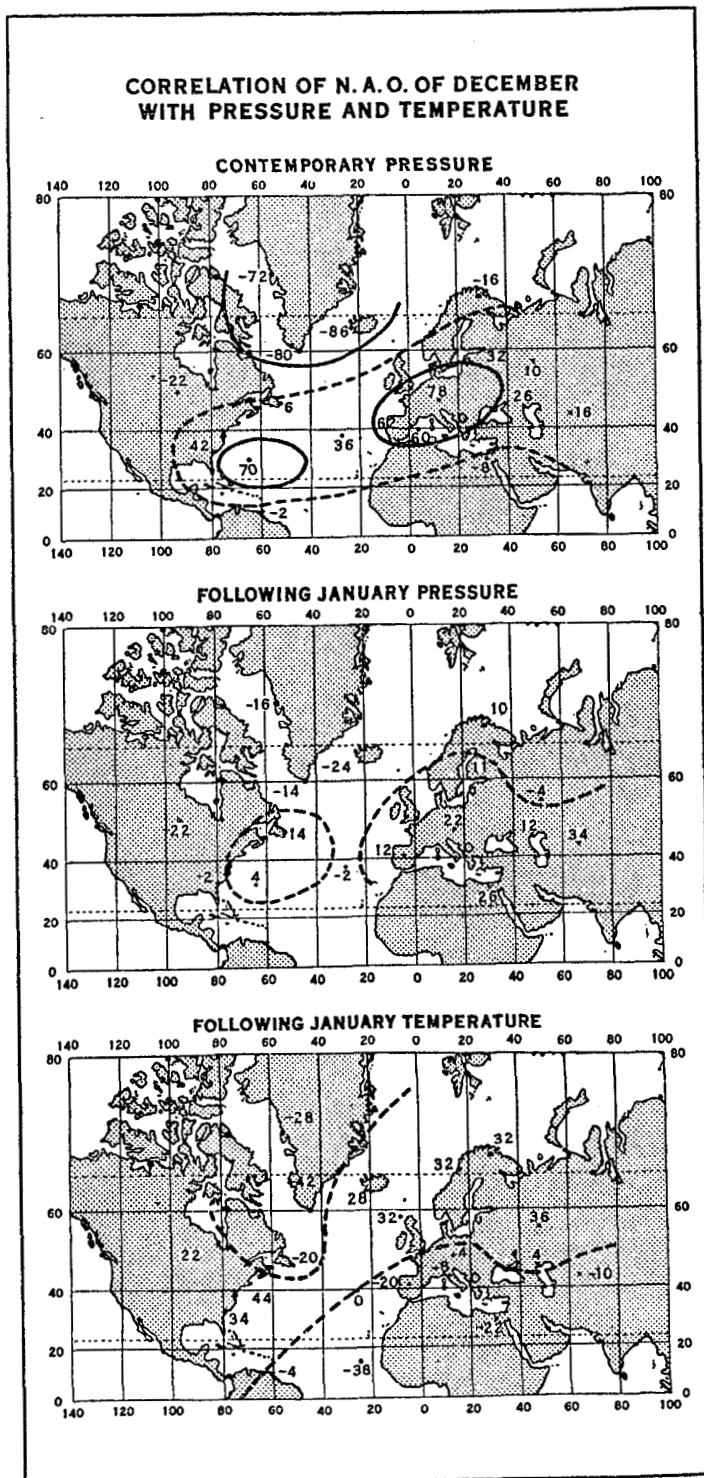
A subsequent paper by Walker (64) is essentially a review of his own and of others' work, but it contains some new and interesting charts, reproduced here. Figure 7 shows that the North Atlantic oscillation has

only a small persistence from December to January, which is further evidence that this oscillation can be only a secondary tool in forecasting. Figure 8 shows the correlations between December to February pressure, temperature, and precipitation, with the preceding June to August value of the southern oscillation. Thus they give the degree to which these elements may be forecast from the observed oscillation, assuming that the coefficients will hold reasonably well in the future. Since the coefficients are in no way chosen for their large size, but merely result from the definition of the oscillation which was arbitrarily chosen to give the best contemporary representation, there seems to be no reason for doubting the validity of this assumption. On the temperature chart there are 87 coefficients based on 30 or more years' data; I have tabulated below the number of these in

each magnitude group, without regard to sign. The number of high coefficients is much greater than would be given by the normal distribution for chance coefficients.

0.00	0.02	0.04	0.06	0.08	0.10	0.12	0.14	0.16	0.18	0.20
6	2	7	2	5	3	3	6	2	5	6

0.22	0.24	0.26	0.28	0.30	0.32	0.34	0.36	0.38	0.40	0.42
0	2	2	2	0	3	2	6	1	1	0

0.44	0.46	0.48	0.50	0.52	0.54	0.56	0.58	0.60	0.62	0.64
1	4	3	0	1	1	2	2	3	1	0

0.66	0.68	0.70	0.72	0.74
0	1	0	1	1

CORRELATION OF S. O., JUNE-AUGUST WITH FOLLOWING DECEMBER-FEBRUARY

PRESSURE

TEMPERATURE

RAINFALL

FIGURE 8.—Reproduced from Walker (64).

The temperature chart shows a large area in western Canada with coefficients of about −0.6. In the same area the precipitation chart gives values of about 0.5, while in the central United States it gives values of about −0.35. The rainfall coefficients would probably be larger if regions were used instead of individual stations.

10. *Periodicities.*—Brief mention should be made here of Walker's three papers on periodicity (42, 47, 54). The first two are largely, and the last entirely, concerned with the statistical theory of periodicity; in this chapter only the meteorological applications will be discussed.

In the first of these Walker says:

The natural oscillation which appears on physical grounds to have most likelihood is that of 2 years' period in the North Atlantic; reversal in alternate years is suggested in the data of ice as well as of temperature and pressure, especially during the years from 1877 to 1903. Using the pressure data of Iceland as an index we find that correlating each year's pressure with the next gives from 1877 to 1903 a coefficient of −0.26; but from 1903 to 1923 the coefficient is +0.48, and for the whole period from 1877 to 1923 the coefficient is −0.02 with a probable coefficient as large as 0.08 due to pure chance. Hence the evidence for a 2-year period is inadequate.

He treats Darwin pressure in more detail, giving a correlation table for each quarter with various later quarters for the period 1882–1923. For all quarters he gets—

Years later:	1½	1¾	2	2¼	2½	2¾	3	3¼
Correlation	−0.10	−0.12	−0.12	−0.02	0.12	0.20	0.24	0.26

	3½	3¾	4	4¼	4½	4¾	5
	0.16	0.12	0.04	0.02	0.00	0.00	0.10

The probable value of the greatest of 15 chance coefficients being 0.15 it is unlikely that luck would produce four amplitudes greater than this, with two of them 0.24 or over.

The applications in the second paper are based on Brunt's "Periodicities in European Weather." He first discusses the rainfall of Milan, for which Brunt had analyzed 90 periods exceeding 12 months but not over 35 years. Of these Walker finds 75 to be independent, but none of the amplitudes are as great as the probable greatest to be expected from a random series when analyzed for 75 periods. Similarly for Padua, London, Edinburgh rain and Edinburgh pressure Brunt's greatest amplitudes are about what would be expected from chance; for Paris pressure the amplitudes are about 50 percent greater. The greatest amplitudes for temperature in six cities are all larger than the most probable ones due to chance. At Stockholm the greatest not exceeding 10 years is five times as large, with an amplitude of 1.24°C. This is a period of 12½ months (which, according to Brunt, has two independent periods between it and 12 months), which is also the most pronounced one at London, Paris, and Vienna. The following temperature periods also have amplitudes appreciably larger than the largest chance one: 13 months at Edinburgh, Stockholm, London, Berlin, Paris; 23 years at London and Edinburgh; 17½ years at London.

Even if the above periods are real, as Walker believes them to be, the amplitudes are all too small for them to be useful in forecasting.

The winter monsoon of Asia, which is due to the intense continental winter HIGH centered at 40° N., merely strengthens the normal trade wind regime in the northern Indian Ocean. During this season the Indian Peninsula experiences dry easterly winds. In northern India the prevailing wind is westerly and there is some precipitation accompanying the passage of winter depressions from west to east. (See 25). At many stations these cause a secondary maximum in the annual precipitation curve which is of great importance to agriculture. Most of India has three seasons, of which this is the first.

The second is the hot season which occurs in early spring before the advance of the southwest monsoon.

is south and continues up the Ganges Valley from the southeast. The monsoon wind and rain reach southernmost India and Ceylon during the last week of May, Bombay and Calcutta early in June, and most of northern India by July first. The southwest monsoon is not particularly strong or steady, but is of large horizontal and vertical (over 3 kilometers) extent and carries northward an immense quantity of warm moist air. This moisture is precipitated in great quantities as orographic rain and to a lesser extent over the plains in convective showers.

Although the general picture is given above, the normal annual amount and time of maximum rainfall are quite

MEAN PRESSURE FOR JULY
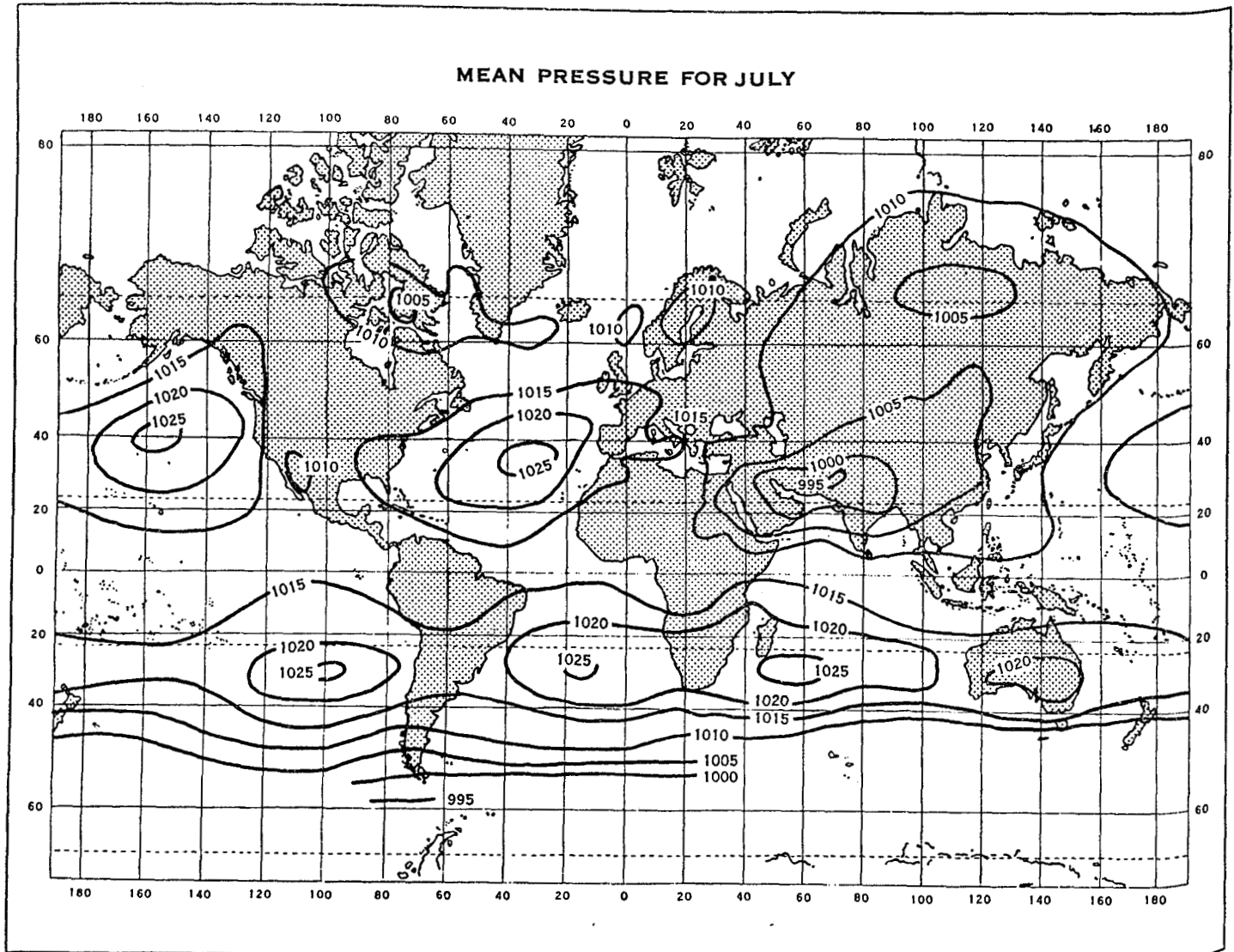
FIGURE 10.—Reproduced from Brunt's Meteorology.

During this period the summer LOW is forming in northwest India and Afghanistan.

The third is the rainy season of the southwest monsoon. The southeast trades in the southern Indian Ocean are uniformly intense throughout the year, the belt of trades being only a few degrees further south in southern summer than in winter. In winter they extend northward to the equator and from there gradually curve eastward and continue as the southwest monsoon, so that no equatorial trough exists in the Indian Ocean in this season. In the Arabian Sea and over the peninsula the monsoon blows from the southwest, but in the northern Bay of Bengal it

different for the 33 rainfall divisions of India. Greatest rainfall occurs on the mountains along the west coast of the Peninsula (Konkan, 108 inches; Malabar, 100 inches); and on those of eastern India (Lower Burma, 126 inches); least occurs in the central and eastern Peninsula (Madras Deccan, 25 inches) and in northwest India (Baluchistan, 8 inches); the figures are annual values. Individual stations would show much greater differences.

The region "India Peninsula" used in Walker's studies covers eight divisions between latitudes 14° N. and 24° N., or roughly the northern and larger part of the Peninsula. (See below under Forecasting.) The normals for

the period June to September range from 22 inches for Gujarat to 93 inches for Konkan. In each of these divisions the rainiest month is July, except that the 3 months July to September in north and south Hyderabad and the 4 months July to October in Madras Coast North are nearly equally rainy. (36, 37).

Southernmost India and western Ceylon have two rainy seasons: April to July, and October to November. The first part of the spring rain comes with the northward migration of the equatorial trough, and heavy rains are received with the outbreak of the monsoon, but relatively little occurs during the second half of the southwest monsoon. In October the weakened Asia summer LOW is found in the Bay of Bengal, which accounts for the fall rain here and on the Madras Coast North. By November the LOW has moved farther south and spread laterally to form the equatorial trough, but is still far enough north for equatorial rain to continue in southernmost India and in Ceylon. The steep east coast of Ceylon has its heaviest rain in winter during the northeast monsoon.

3. *Africa.*—Zanzibar has its heaviest rain in March to May, and in November. During southern winter the combined effect of the India and South Africa monsoons produces prevailing southerly winds there, and during southern summer they produce northerly winds, neither of which is conducive to rain. In spring and fall, on the other hand, winds are weakened and easterly, so that more truly equatorial conditions exist, which, together with the eastern exposure of Zanzibar, favor rain there. There seems to be a conflict in the Indian Ocean region between the monsoon tendency and a tendency toward true equatorial conditions. Thus, if the second tendency is unusually pronounced in April and May, there is more rain than normal at Zanzibar and, according to Walker's early belief (1), the tendency would persist so that the southwest monsoon would be later and weaker than normal in India.

November to March are the rainiest months at Seychelles Islands. During this time the doldrum belt is just south of the islands and the winds are northwest or calm and variable. Hence the rain is of the equatorial type. The southeast trades prevail the rest of the year. The southeast trades prevail at Mauritius in all seasons, and December to March is the rainy season. Rhodesia has its rainy season in summer, the rain being of the convective shower type. The moist air is supplied by the monsoon tendency which diverts the trade winds so that they blow from the northeast across the coast. The summer rain of central and eastern South Africa is similar to that of Rhodesia; orographic rain is more important because the winds are more easterly, the monsoon merely strengthening the trades. The southernmost part of this region has an equal amount of rain in winter due to the passage of storms from the west. Western South Africa has only winter rain.

4. *East Indies and Australia.*—The islands lying between Asia and Australia experience a reversal of wind between summer and winter according to the monsoons of these two continents. In March to April and again in October to November, at the change of the monsoons, a trough of low pressure coincides fairly closely with the Equator, and flat land near the Equator has two rainy seasons. A good example of this is Pontianak, on the Equator in western Borneo, maxima in November and March. At a greater distance from the Equator flat land has a summer rainy season of the tropical type. Much of the land, however, is mountainous and receives its rain during one of the monsoons, so that the distribution of rainfall throughout the islands is very complicated.

In most parts of Java the rainiest month comes during December to February, and this summer rain is largely of the orographic-monsoon type. The winter monsoon is much drier at Java because it blows Equatorward and has had only a short path over water from the dry continent of Australia, and is also weaker, so it gives little rain.

Further east the southeast monsoon is no longer dry because it comes from east of Australia. Amboina has rain maxima in both December and June, Makassar in January and June, the latter maximum being much greater in both cases. On the other hand Menado has a single maximum in January, so it must be sheltered from the southerly monsoon, the June to August rain being of the equatorial type.

Siam has wet summers and dry winters, with maxima in May and September. This rain seems to be largely of the equatorial type, like that of Ceylon, and is lessened during the height of the southwest monsoon.

The Philippines are under the influence of the northeast and southwest monsoons. The rainfall comes in summer or in winter depending on the orographic conditions of each locality. North Borneo has a similar climate; maximum rain occurs at Sandakan in July.

The Australia summer LOW, which may be called a southward intensification of the equatorial trough, is centered on the northwest coast and from Darwin eastward the northwest monsoon is the most frequent wind. During this season the trades in a central zone are intensified by the monsoon circulation, and on the east coast of Queensland appear as a northeast monsoon. The south coast extends into the westerlies. In the rainfall region "northeast Australia" the rainy season is December to March and is definitely associated with the summer monsoon; the winter is very dry except in southern Queensland. The winter HIGH is centered somewhat south of the middle of Australia; the northern and larger zone then experiences the trades and the southern zone the westerlies.

As mentioned before, pressure in Australia has a negative correlation with the southern oscillation in both seasons. Thus, while the summer monsoon is strengthened, the winter monsoon is weakened, which is accompanied by an increase in southern Australia rain due to a northward extension of the storm tracks. This combines with the decrease of the summer monsoon in the China region to produce a partial replacement of the monsoon by normal equatorial conditions, resulting in higher temperature in northern Australia and Papua and more rain at Pontianak and Menado and in Siam (fig. 3). The area of excess rain includes Amboina, whose June to August rain was stated to be of the monsoon type, so this station does not fit into the scheme. But the Philippines, whose rain is more definitely of the monsoon type, form a strong negative area. In December to February on the other hand, as shown by figure 4, monsoon rains are increased and equatorial rains decreased in this region.

5. *Pacific Ocean.*—At Hawaii the trades prevail in summer, but in winter they are often interrupted by the westerlies. In general the maximum rainfall comes in winter during the westerlies, although the trades give orographic rain in all seasons and in summer the west sides of the islands receive convective rain. The increase of winter rain with the southern oscillation is evidently associated with a southward displacement of the Aleutian LOW and a closer approach of the storm tracks, which agrees with the opposition between the southern and the North Pacific oscillations.

Ocean and Malden islands lie in a longitude where the doldrums are usually lacking and the trades prevail throughout the year. This evidently prevents the normal equatorial type of rain from occurring, with the result that the annual rainfall is only about 50 centimeters, though more seems to fall on the ocean. Occasionally the trades must break down, for Malden had 125 centimeters in 1900 and 161 centimeters in 1905. In both June to August and December to February the southern oscillation must increase the trade winds here, although this tendency is only faintly indicated by figures 3 and 4. Increased trades are not only prejudicial to rainfall, but also give lower land temperatures by counteracting the high insolation, and probably also lower water temperatures.

A thousand miles southwestward, at Samoa and Raratonga, the climate is entirely different. These are under the remote influence of the Australia monsoon so that the trades, which prevail the rest of the year, are often interrupted in summer, and the northerly monsoon component maintains nearly constant high humidity. The rainfall, which is heavy, is of the normal tropical (convectional) type with maximum in January and February while temperature is highest in December and March. With the southern oscillation in both seasons the monsoon tendency gives more frequent interruptions of the trades, allowing more rain. In summer the increased rain is accompanied by lower temperature.

6. *South America.*—In South America there are three rainfall regions which need to be considered. The first of these is Chile, which has decreasing winter rain from south to north, and also summer rain in the southernmost part. The winter rain is due to the passage of storms in the belt of westerlies, of course aided by the orography. Walker's region "Chile rain" lies in the northern part of this belt, so its decided negative coefficient with the southern oscillation June to August probably indicates a southward displacement of the storm tracks. The extension of this negative region to Punta Arenas (fig. 4) does not seem consistent, as this station must lie near the axis of the storm tracks.

The role of "South America rain" is not clear. The moist air for this rain must be brought from the tropical Atlantic by the tendency toward a summer monsoon in South America. Pressure is neutral in South America in the zone of sub-tropical high pressure with the southern oscillation December to February (fig. 4). There are no coefficients for the Atlantic in this zone. However, if the latter region is negative, as seems probable from the surrounding distribution, the oscillation involves a decrease in the monsoon gradient and hence would account for the decreased "South America rain."

In Ceara (Fortaleza and Quixeramobim) the southeast trades prevail at all seasons. The rainy season is from February to June. Hann suggests that this fall rain is due to the land temperature being lower than the water temperature, but this seems to be an entirely unsatisfactory explanation.

IV. FORECASTING THE MONSOON RAIN OF INDIA

1. *Early forecasting.*—During the first few years Walker was in India he advanced the forecasting procedure to the point where the following factors were considered important:

(a) Late and heavy snowfall in the north and west of India had long been considered prejudicial to the following monsoon. The accumulation at the end of May in a locality not defined in detail was tabulated by Walker ((12) table 1) for 1876–1908, on a numerical scale of departures ranging from −2 to 3.

(b) Heavy April-May rain at Zanzibar and Seychelles is prejudicial.

(c) In South America "the pressure departure associated with an abundant Indian monsoon is strongly positive during the monsoon and decidedly positive for some months previous to June," (12).

(d) High pressure in spring in and around the Indian Ocean, particularly in Mauritius and Australia, was considered prejudicial.

(e) High pressure in India during the previous calendar year was at first considered favorable, but the correlation fell to 0.17 for 1865–1908 and was abandoned (12). Giving the first 10 years two-thirds weight, the same coefficient became 0.25 for 1865–1912, so it was further studied (26).

(f) Winds observed by ships in the equatorial part of the Indian Ocean in May give indications of the advance of the monsoon.

(g) The monsoon rain in Abyssinia starts in May, so its departure from normal in time of setting-in and strength may be expected to hold for India also. This rainfall information is supplemented by the height of the Nile.

Memoranda have been issued every year, I believe, from 1904 to the present, on about June 7 concerning the monsoon rain of June–September, and on about August 7 concerning the monsoon rain of August–September. The above factors as observed up to the last of May were used in the June forecast, and in general the same factors extended to the last of July were used in the August forecast.

The first regression equations published by Walker (4, 5) for the prediction of the monsoon rain in June to September and in August to September need not be discussed here. The first of these was soon replaced by the "formula of 1908" (12). This formula is for the whole of India, the rainfall departures being the mean of the provincial departures weighted according to their areas, a provincial departure being the simple average of the departures at all stations in the Province. These departures are tabulated for 1841–1908 ((11), table 1), but are unreliable before 1865. The formula is:

(rainfall) = −0.20 (snow) − 0.29 (Mauritius pressure May) + 0.28 (South America pressure) − 0.12 (Zanzibar rain April to May)

"South America pressure" is the mean of Buenos Aires, Cordoba, Santiago; the months March, April, May are weighted ½, 1, 1, respectively. Here, as in subsequent formulae, quantities in brackets are proportional departures (ratio of departure to its standard deviation), and the formula is based on the following coefficients:

	2	3	4	5
1. India rain	−0.36	−0.36	0.42	−0.31
2. Snow (1876–1908, 12 of these ¼ weight)		.09	−.37	.31
3. Mauritius (1875–1908)			−.12	.15
4. South America (1865–1908)				−.32
5. Zanzibar (1892–1908, 11 previous scattered years ½ weight)				

In regard to snow in northern India, it was early believed that an unusually large cover in late spring led to local high pressure, with dry northerly winds that would be prejudicial to monsoon rainfall. But the coefficients of snow with South America and Zanzibar are of the same magnitude as those of the latter with the monsoon, so snow, like these other two indices, must at least in part

give information of the character of the general circulation which will later produce the monsoon rain. We have seen that South America pressure is representative of the southern oscillation, and that Zanzibar rain probably indicates the degree of development of the monsoon circulation. Similarly late snow indicates a persistence of winter conditions and a weakened tendency toward development of the monsoon.

The average number of years of data on which the formula above is based is 30, and the multiple correlation coefficient is 0.58. The correlation between the predictions of the formula and the actual rainfall for 1909–21 is 0.55 (35). For 1909–27 it is 0.56 (51).

In Walker's 1914 paper (26) he discusses, besides the influence of previous pressure in India, the influences of May temperature in India and of Antarctic icebergs. In spite of the expectation that high spring temperature, especially in northern India, would cause a well-developed monsoon, no relation was found. The ice data give the annual number of bergs reported in the South Indian, the South Atlantic, and the South Pacific during a broken series of years from 1885–1912. No relation is found for ice in the Indian Ocean, but years of many bergs in the vicinity of Cape Horn were usually years of high pressure in South America and heavy monsoon rain in India. Since these relations are for contemporary calendar years of bergs and pressure, they do not necessarily have any forecasting value. Table 1, however, shows that icebergs in the South Pacific, December to February, have a correlation of 0.38 with the southern oscillation in December to February and 0.28 with it in the following June to August. Ice conditions were not taken into account in any of the forecasts.

2. *The 1919 Formulae.*—A beginning was made in 1913 with the examination of the factors which might determine the geographical distribution of the rainfall in India (35). The results are given in Walker's paper of 1922. In order to study the effect of previous pressure in India in detail, India was divided into 16 pressure districts, each represented by 3–18 stations. Table II of the paper gives the coefficients of May pressure in each of these districts with the monsoon rain (June to September) in each of the 33 rainfall subdivisions for 1875–1913. The table also gives the coefficients of the rainfall subdivisions with May pressure of India as a whole, with May pressure at Mauritius, and with April and May pressure in South America (Buenos Aires, Cordoba, Santiago) for 1875–1913; and with snow accumulation at the end of May, with May rain at Zanzibar and at Seychelles (both 1891–1913), with May rain in southern Ceylon (Galle, Kalutara, Ratnapura, 1875–1918), and with the previous rain in Java ("sum of the monthly percentages from October to February," 1880–1919).

The coefficients with the pressure districts are mostly or entirely insignificant, but the coefficients of the external factors with the rainfall subdivisions made it possible to form two large regions such that each is homogeneous as regards forecasting. "Peninsula" consists of Gujarat, Central Provinces (2 subdivisions), Konkan, Bombay Deccan, Hyderabad (2), Madras Coast North. "Northwest India" consists of United Provinces West, Punjab (2), Kashmir, Northwest Frontier Province, Rajputana (2). The subdivisions are weighted according to their areas, except that Punjab Southwest is given only half that weight.

Walker determined the formula, (Peninsula)=0.44 (South America) −0.29 (Zanzibar) −0.41 (Java), on the basis of the coefficients:

	2	3	4
1. Peninsula rain, June to September	0.47	−0.48	−0.45
2. South America pressure, April and May (1875–1919)		−.20	.07
3. Zanzibar rain, May (1891–1919)			.24
4. Java rain, October to February (1880–1919)			

Similarly for northwest India, (Northwest India)=0.35 (South America) −0.21 (snow) −0.14 (Zanzibar) −0.13 (Ceylon):

	2	3	4	5
1. Northwest India rain, June to September	0.50	−0.38	−0.24	−0.29
2. South America pressure, April and May (1875–1919)		−.34	−.20	−.41
3. Snow accumulation (1876–1919)			.21	.15
4. Zanzibar rain, May (1891–1919)				−.07
5. Ceylon rain, May (1875–1919)				

The multiple correlations for these two formulae are 0.73 and 0.57, respectively.

It will be noticed that Mauritius May pressure does not enter into these formulae, for its coefficient fell so that with Peninsula rain it was only −0.21 for 1875–1919. Although the southern oscillation June to August has a coefficient with Mauritius of −0.56 in June to August and −0.27 in March to May, which seems to account for the weak relation between Mauritius and India rain, one would naturally expect high pressure at Mauritius to be associated with well-developed southeast trades and monsoon, and hence excess rain in India. Thus the physical connection is not clear, and Mauritius does not seem a reliable indicator for India rain. Nevertheless it is used in the formula for Malabar.

The snow accumulation at the end of May in northern India is naturally most important for northwest India where it has some direct effect, its coefficient with Peninsula rain being only −0.16 for 1876–1919.

Walker has attempted no explanation for the connection with Java rain. Java has a coefficient of only −0.12 with the southern oscillation in following June to August, and, although it has 0.62 with the oscillation in December to February, the latter has a persistence till June to August of 0.20. From another point of view, heavy rain in Java indicates a strong northerly monsoon, and it may be reasonable that a strong northerly monsoon should be followed by a strong southerly one in the same region (slightly substantiated by coefficients with Java rain of 0.18 for Darwin pressure June to August 1882–1921, and 0.09 for Upper Burma rain June to September 1880–1919), and hence a weaker monsoon in India since there is evidence that in June to August the strongest part of the monsoon is often shifted either eastward or westward.

It has already been indicated that Ceylon rain is largely of the equatorial type, so it is reasonable to find it associated negatively with the rain of most of India.

For forecasting in Upper Burma Walker found:

	2	3	4
1. Upper Burma rain, June–September	0.22	−0.29	−0.47
2. India pressure, May (1875–1919)		−.21	.39
3. South America pressure, April–May (1875–1919)			.03
4. Seychelles rain, May (1891–1919)			

(Upper Burma)=0.43 (India) −0.18 (South America) −0.63 (Seychelles.)

The multiple correlation is 0.67. This formula is logical if the southern oscillation represents a westward displacement of the monsoon, resulting in subnormal rain

in Burma when India has an excess. Walker says that Seychelles is far enough east to be in the path of the monsoon winds destined for Burma, and so gives the same indications for Burma that Zanzibar gives for India. The June to August rains of Zanzibar and Seychelles have coefficients of −0.24 and 0.22 respectively with the southern oscillation.

Walker gives similar formulae for the summer rains in the subdivisions of Mysore and Malabar.[3]

Formulae are also given for forecasting the August to September rain in the Peninsula and in Northwest India on the basis of data up to the end of July.

I cannot find that subsequent verifications have been published for any of these formulae from the paper of 1922.

3. *The 1924 Formulae.*—After the first two "World Weather" papers were prepared Walker published new formulae (40) which included new indices. A table gives the correlations between Peninsula rain and 41 factors 0–4 quarters previous. He first gives a formula for Peninsula rain based on data including 1921, and then the following "1924 formula":

	2	3	4	5	6	7
1. Peninsula rain, June to September	−0.38	0.44	−0.38	−0.36	−0.42	−0.50
2. Cape Town pressure, September to November (1876–1923)		−.16	.38	.10	.06	.14
3. South America pressure, April to May (1875–1923)			−.54	.04	−.16	−.40
4. Dutch Harbor temperature, December to April (1882–1919)				.16	−.04	.18
5. Java rain, October to February (1880–1923)					.08	.22
6. Zanzibar district rain, May (1893–1923)						.16
7. Southern Rhodesia rain, October to April (1899–1922)						

(Peninsula) = −0.22 (Cape Town) +0.20 (South America) −0.12 (Dutch Harbor) −0.24 (Java) −0.32 (Zanzibar district) −0.26 (Southern Rhodesia)

Zanzibar district consists of Banani, Dar-es-Salam, Zanzibar. The multiple coefficient is 0.76.
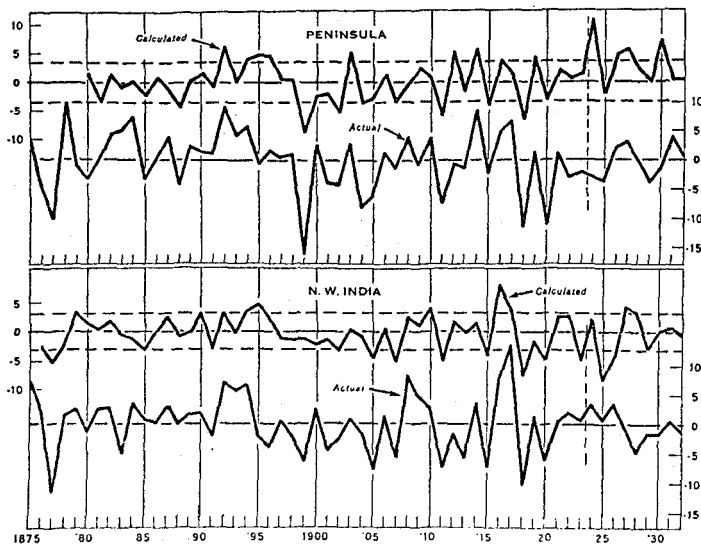
FIGURE 11.—Actual rainfall departures in India, June–September and those calculated by 1924 formulae, in inches. Reproduced from Walker (64).

Walker hints at no explanation for the relation with pressure at Cape Town 9 months before, but it is suggestive of an ocean temperature effect of some kind. Dutch Harbor temperature is closely associated with the North Pacific oscillation, whose influence in winter is

[3] The forecasts for Burma, Mysore, and Malabar were not sufficiently reliable and were soon discontinued, according to letter from Walker dated September 3, 1938.

strongly felt as far west as Java; but, since the oscillation is not defined for spring or summer, it is impossible to suggest any exact physical connection with India rain. Light rain in Southern Rhodesia probably indicates a weak summer monsoon tendency in southern Africa, but again no good reason can be offered why this should be followed by a strong monsoon in India.

In this paper Walker gives a few correlations of pressure gradients with Peninsula rain June to September. Only one of these is appreciable; it is −0.72 with the July pressure difference of district IX (Gujarat) less district IV (Bihar and Chota Nagpur). In other words pressure is relatively high in eastern India and low in western India during summers of heavy rain, which agrees with my statement that the southern oscillation June to August involves a westward shift of the Asia LOW rather than an intensification.

A table gives the correlations between northwest India rain and 23 factors 0–4 quarters previous. From these Walker finds:

	2	3	4	5	6	7
1. Northwest India rain, June to September	−0.42	−0.38	0.50	−0.42	−0.52	−0.40
2. Equatorial pressure (1895–1922)		.02	−.46	.32	.28	.44
3. Cape Town pressure, September to November (1876–1923)			−.16	.26	.14	.32
4. South America pressure, April and May (1875–1921)				−.48	−.40	−.34
5. Dutch Harbor temperature, March to May (1882–1919)					.08	.22
6. Southern Rhodesia rain, October to April (1899–1922)						−.06
7. Snow accumulation end of May (1876–1921)						

(Northwest India) = −0.06 (equatorial), −0.14 (Cape Town), −0.04 (South America), −0.24 (Dutch Harbor), −0.48 (Southern Rhodesia), −0.30 (snow).

"Equatorial pressure" is the mean of the departures at Seychelles and at Zanzibar in February to March, at Batavia in January to April, and at Darwin in March to May. The correlations between these are all high. The months were chosen which gave the highest correlations with northwest India rain. This formula gives a multiple correlation of 0.76.

The changes from the formula published in 1922 are that Zanzibar rain and Ceylon rain have been dropped, and four new indices have been added which apply to conditions a considerable time before the monsoon. No physical basis offers itself for the new indices, so this more purely empirical formula does not seem to promise more than the older one.

Walker ((64,) fig. 11) published a verification of the above two "1924 formulas," which is reproduced here as figure 11. The "limits" drawn in this diagram are such that, if a forecast is made only when the indication is outside the limit, the forecast should be correct in sign of departure 4 times out of 5. Walker says that forecasts should be issued only when there is this 4 to 1 chance of success. The verification here is for 1924–32, so that the two formulae give 18 cases. In 8 of these the indication is outside the limit, but only 2 give the correct sign.

The 1924 paper also gives formulae for the prediction of the summer rain in northeast India and of the August to September rain in the Peninsula and in northwest India.

The memoranda issued each June and August containing the monsoon forecast have not, with a few exceptions, included any reference to the formulae. They have consisted of a statement of the conditions up to date (such as those used in the formulae), followed by a discussion of their significance, and finally they give forecasts in general terms for the several major divisions of India as to whether

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the rains will be early or late and above or below normal. Walker says (64): "After careful scrutiny I estimate that of the forecasts issued before the monsoon periods (i. e., the June memoranda) from 1905 to 1932 two-thirds were correct." We may reasonably conclude that the forecasting methods in India have a small but still appreciable success. But the verification of general forecasts is always subject to uncertainty, and therein lies the value of the numerical forecasts given by the formulae. The complete success of the "formula of 1908" up to 1927 was good evidence in favor of the methods used in India. The verification published elsewhere in this volume, however, shows that the "formula of 1908" has fallen down completely for the period 1922–36. The "1919 formula" and "1924 formula" for Peninsula rain, June to September, were also found to show only slight success. Unfortunately the verification did not cover Walker's other formulae mentioned above.

V. FORECASTING OTHER THAN FOR THE MONSOON RAINS OF INDIA

1. *Winter precipitation in northern India.*—Seasonal forecasts of the winter precipitation in northern India have been issued regularly, just as the two summer forecasts. These have been based chiefly on the relation that, if the precipitation is above normal in late fall, it will continue so throughout the winter. In the beginning the memoranda were issued early in December for the period December to February, but these early forecasts were unsuccessful. It was found that December conditions gave a better index of the succeeding weather, so the later memoranda were issued in early January to cover the period January to March.

The factors utilized in the early forecasts are the following:

(a) The persistence of late fall precipitation throughout the winter. Walker (13) gives a table of departures of rainfall in December and in January to February in Punjab and Northwest Frontier Province for 1890–1909. This shows a persistence of sign in 15 of the 18 cases.

(b) Heavy rain in late fall at Zanzibar and Seychelles is favorable. Walker says (3):

There is nothing a priori improbable in a relationship between the rainfall at Zanzibar and Seychelles and the subsequent winter precipitation in northern India. Excess of the former would imply increased ascensional movement at the Equator and increased flow in the upper atmosphere in directions away from the Equator; and as it is well known that the cold weather storms occur in the higher air levels it is natural that their vigour should be affected by an increase in the supply of air from the Equator.

(c) It had been shown by Eliot in 1893 that when the precipitation is above normal the pressure difference between the plains and the hill stations above them tends to be above normal. Beginning in 1907 (6) a temperature correction was applied to the pressure differences. The correlation between the corrected pressure difference in November with the January rainfall is 0.5 for 1876–1906. Since the nature of the correction is not stated, it is impossible to discuss the significance of this relation.

Walker and Hem Raj (25) found that dry years in northern India were associated with much disturbed weather in the southern Bay of Bengal. "The phenomenon is apparently due to a displacement or an extension northwards of the equatorial belt of squally weather; and is frequently associated with or precedes a marked shift northwards of the usual path of winter depressions." The correlation for 1875–1911 between Port Blair December rain and January and February rain in northwest India is −0.21, for January alone it is −0.42.

In a later forecast Walker (34) says that strong upper winds at Agra are associated with stormy winters, so that, due to the persistence tendency, stronger than normal winds in the late fall are usually followed by more winter rainfall than normal.

In the formula developed for the winter precipitation (40), the departure of rain in northwest India, r, is defined as the mean of the monthly departures (weighted for areas) of January to March in Punjab, Northwest Frontier Province, Sind, Rajputana, Gujarat, and of January to February in the United Provinces. The winter snow of the western Himalayas, s, is graded on a scale of −3 to 3. The standard deviations of r and s are 0.97 and 1.62, and the winter precipitation is defined as—

$$p = r + 0.4s$$

The following formula was developed:

	2	3	4	5
1. p	0.42	0.56	−0.28	0.52
2. Seychelles pressure, November–December (1895–1920)		.14	−.18	.16
3. Western rain, December (1892–1920)			0	.34
4. Fort Blair rain, December (1876–1920)				−.10
5. Rain: sum of Seychelles November–December, Zanzibar December (1892–1920)				

$(p) = 0.28$ (Seychelles pressure) $+0.42$ (western rain) -0.20 (Port Blair) $+0.32$ (Seychelles and Zanzibar rain).

"Western rain" is based on the "Daily Weather Report" stations in Northwest Frontier Province, Kashmir, Baluchistan, Persia. The multiple correlation for the formula is 0.76.

2. *Nile flood.*—In several papers Walker has published formulae for seasonal forecasting in certain regions outside of India. The first of these was for the prediction of the Nile flood (12), of which the percentual departures were tabulated for 1737–1800 and 1825–1908 (11).

Pressure at Cairo was in use by Lyons for forecasting the flood, but Walker found this pressure in April and May to have only slight influence. He determined the formula:

	2	3	4
1. Nile flood	0.49	−0.44	−0.35
2. South American pressure (1865–1908)		−.32	−.37
3. Zanzibar rain, April and May (1892–1908, 11 previous scattered years ½ weight)			−.31
4. Snow (1876–1908, 12 of these ½ weight)			

(Nile) = 0.35 (South America) −0.29 (Zanzibar) −0.13 (snow). The multiple correlation is 0.59.

3. *Australia.*—Northern Australia receives summer monsoon rains, and, due to the persistence of the southern oscillation from southern winter to summer, one should expect to be able to forecast the rains several months in advance. In the first formula Walker developed (2) for the purpose, however, two of the three indices apply to conditions immediately before the heavy rains begin. This formula, which follows, is for the whole of Australia except the extreme south, though it is based on at most 22 stations:

	2	3	4
1. Australia rain, August to July	−0.45	−0.31	0.37
2. Australia pressure, October to November		−.08	−.33
3. Mauritius pressure, October to November			.04
4. India rain, June to September			

(Australia rain) = −0.39 (Australia pressure) −0.35 (Mauritius) +0.25 (India).

All coefficients are for the seasons ending in 1876–1904; the multiple correlation is 0.61. For 1905 the formula indicated no departure and there was actually a large defect; in the following 3 years both indicated and actual departures were small, but agreed in sign only once.

Later Walker and Bliss (59) gave a new formula for the summer rain. This time the stations are the same as those used in the definition of the southern oscillation; their number increases from 8 in the season ending in 1871 to 29 in 1892.

	2	3	4
1. Northeast Australia rain, October to April	0.62	−0.74	0.52
2. Honolulu pressure, March to August (1883–1927)		.58	.48
3. Darwin pressure, June to August (1882–1927)			−.50
4. Cordoba and Santiago pressure, June to August (1870–1927)			

(Northeast Australia) = 0.25 (Honolulu) −0.53 (Darwin) +0.14 (South America)

The multiple correlation is 0.79

4. *America.*—In the same paper Walker and Bliss give a formula for the prediction of winter temperature in southwest Canada (Calgary, Edmonton, Prince Albert, Qu'Appelle, Winnipeg).

	2	3	4	5
1. Southwest Canada temperature, December to February	−0.56	0.62	−0.60	0.58
2. Honolulu pressure, June to August		−.62	.54	−.56
3. Darwin pressure, June to August			−.54	.72
4. Monsoon rain				−.50
5. Madras temperature, June to August				

(Southwest Canada) = −0.15 (Honolulu) +0.24 (Darwin) −0.30 (monsoon) +0.17 (Madras).

"Monsoon" is the mean of the proportional departures of the June to September rains in the Peninsula and in Northwest India and of the Nile flood. The coefficients are based on the seasons ending in 1885–1928. The multiple correlation is 0.72. A similar formula omitting Madras gives 0.71. The indices are closely connected with the southern oscillation June to August, which persists to December to February, and in the latter period the oscillation is associated with low temperature in western Canada. (See figs. 3 and 4.)

A formula is also given for Dawson winter temperature, which involves the same principles:

	2	3	4	5
1. Dawson temperature, December to February	−0.56	−0.54	0.54	0.48
2. Honolulu pressure, June to August		.58	−.26	−.62
3. South American pressure, June to August			−.24	−.52
4. Zanzibar pressure, June to August				.28
5. Darwin pressure, June to August				

(Dawson) = −0.26 (Honolulu) −0.26 (South America) +0.39 (Zanzibar) +0.07 (Darwin).

The coefficients are all based on the seasons ending in 1902–1925, and the multiple correlation is 0.72. The forecast for 1926 was normal and an excess of 14° occurred, which was the highest on record; for 1927 the forecast was perfect.

In this same paper by Walker and Bliss a formula is given for predicting the summer rains of South Africa.

In another paper (53) Walker gives the following formula for the rain in Ceara, which he tabulated in percentages of normal for 1866–1926.

	2	3	4	5	6
1. Ceara rain, January to June	0.52	0.38	−0.40	−0.56	−0.40
2. Santiago pressure, June to August (1869–1923)		.52	−.02	.06	.00
3. Honolulu pressure, June to November (1883–1923)			.00	−.06	.32
4. Cape Town pressure, September to November (1875–1923)				.50	.40
5. Southern Rhodesia rain, July to November (1898–1923)					.50
6. St. Helena wind velocity, September to November (1893–1923)					

(Ceara) = 0.44 (Santiago) +0.20 (Honolulu) −0.10 (Cape Town) −0.42 (Southern Rhodesia) −0.22 (St. Helena).

The first three indices in this formula are essentially a measure of the southern oscillation, and their value must lie in the persistence of the oscillation. Though connections with the last two indices seem plausible, the nature of the relations is not suggested. At any rate it is impossible to trace the exact physical connection between the indices and Ceara rain, because not even the nature of the rain is known.

5. *Verifications.*—I cannot find that verifications up to date of the formulae above have been published. But Walker (57) states briefly the results of verifications of certain of the formulae mentioned in this chapter and of the "1924 formulae" for the India monsoon. He mentions only the cases where the indicated departure was great enough to give a 4 to 1 chance of success in forecasting the sign of departure. Of the 12 such cases, which are treated collectively, 6 were correct, 5 wrong, 1 neutral. But it should be remembered that the coefficients on which the formulae are based were selected for their size from the significant factors and hence the multiple correlation "instead of being, say 0.75, is in reality probably between 0.65 and 0.7. It seems wiser to adopt the smaller value * * * and so there must be a larger indicated departure to justify the issue of a prediction, namely 0.63 times the standard deviation instead of 0.56 times it. Accordingly instead of 12 cases there are only 8, out of which 5 are right, 2 wrong and 1 neutral."

VI. SUMMARY

The above report is a summary, to some extent critical, of Walker's various studies, and it would not be worthwhile to condense each item further. For the general scope of his work the reader is referred to the Table of Contents (note that the chapter on climatology is included as a background for understanding Walker's work, and does not represent a contribution by him).

It is best here to focus attention on Walker's most important contribution, namely his studies concerning the three oscillations. The North Atlantic oscillation is a tendency for subnormal pressures in the region of the Icelandic LOW to be accompanied by pressure above normal in the subtropics and vice versa. This oscillation has been numerically defined, for each quarter of the year separately by means of formulae involving quarterly departures from normal of meteorological phenomena at specific stations. The resulting numerical values have in turn been correlated with meteorological phenomena at many stations, the coefficients being plotted on separate charts for each quarter and for each of the elements pressure, temperature and precipitation. (See figure 1 and charts in "World Weather VI".) These charts present in detail the relationships involved in this oscillation. They may be regarded as giving a picture of the most likely deviations from normal over the region within and adjoining the North Atlantic Ocean under

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abnormal conditions. However, the persistence tendency of the North Atlantic oscillation is too slight to be of use in forecasting.

The North Pacific oscillation is similar to the North Atlantic. It has been numerically defined for the winter quarter and the correlations with it are shown in figure 2. It has not been studied for the other quarters.

The southern oscillation is a tendency for high pressure in the South Pacific to be accompanied by low pressure in the Indian Ocean and vice versa. It has been numerically defined for each quarter and the correlations with it are shown in figures 3, 4 and charts in "World Weather VI." Its influence is not limited to the Southern Hemisphere but extends markedly into certain regions of the Northern Hemisphere. The persistence tendency of this oscillation is shown by the following coefficients between the successive quarters.

December to February with March to May_____ 0. 68
March to May with June to August_____ .62
June to August with September to November_____ .82
September to November with December to February_____ .90

Correlations with the oscillation of previous and subsequent conditions are shown in figures 5, 8 and charts in "World Weather VI."

As regards the seasonal forecasting for India by Walker and his predecessors and successors, the success achieved is not striking. However, his studies concerning the oscillations, especially his discoveries of a strong persistence of the southern oscillation and of the influence of this oscillation in the Northern Hemisphere, indicate that similar methods might lead to seasonal forecasts of value for North America.

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VERIFICATION OF THREE OF WALKER'S SEASONAL FORECASTING FORMULAE FOR INDIA MONSOON RAIN

By R. B. Montgomery

In connection with the analysis of the work of Sir Gilbert Walker in seasonal forecasting a verification of some of his forecasting formulae was found advisable, and forms the subject of the present paper. Although Walker himself has published verifications for a few of his formulae up to the years 1921 (1), 1927 (2) and 1932 (3), one by a disinterested person is always to be desired. Furthermore the work is brought up to 1936.

From the two-dozen-odd formulae Walker has published, I have chosen for verification the three which have been published successively for the June to September rain in all or central India.

The first of these for the mean rain of all India, was published in 1910 (4) and is based on correlations of data extending up to 1908. In terms of "proportional departures," i. e. ratio of departure from normal to standard deviation, the Formula of 1908 is:

India rain = −0.20 Northwest India snow accumulation, end of May.
　　　　　 −0.29 Mauritius pressure, May.
　　　　　 +0.28 South America pressure, March to May.
　　　　　 −0.12 Zanzibar rain, April and May.

Walker's suggestions as to the nature of the relations involved may be outlined as follows. Late and heavy snow in northwest India was supposed to be partly the result and partly the cause of an unusual persistence of winter conditions with a consequent delay in the establishment of the southwest monsoon and its rainy season. Heavy rain at Zanzibar in April and May indicates a predominance of equatorial over monsoon conditions, and, supposing this tendency to persist through the following months, would precede a weak southwest monsoon in India. Pressure conditions in the Indian Ocean in May, as represented by Mauritius, were considered dependent in some way on the early character of the southwest monsoon, but the exact nature of this relation were not known. It is easiest to throw light on the relation with South America by referring to what is now known as the southern oscillation (5), which involves a tendency for seasonal departures to occur according to a definite pattern which covers a major portion of the earth. Walker has devised a means of assigning quarterly values to the oscillation, and finds the contemporary oscillation correlated positively with Santiago pressure in March to May and with India rain in June to September, and also finds the oscillation persistent from one quarter to the next. The multiple correlation coefficient for this formula, or the correlation between its "predictions" and the actual rainfall for the years up to 1908, is +0.58. Walker found correlations of observed values with those calculated by the 1908 formula for the periods 1909–27, of +0.55 (1) and +0.56 (2).

Table 1 gives the actual rainfall departures and those from the formula according to my computation. The correlations between these two series for various periods are as follows:

1909–21 _____ +0. 55
1909–27 _____ +. 45
1909–36 _____ +. 32
1922–36 _____ −. 25

These correlations are not the same as multiple correlations for the periods shown. The multiple correlation for the years 1909–21 is +0.58 and the regression equation in terms of proportional departure is:

India rain = −0.22 Northwest India snow accumulation, end of May.
　　　　　 −.31 Mauritius pressure, May.
　　　　　 +.41 South America pressure, March–May.
　　　　　 −.24 Zanzibar rain, April–May.

It is apparent that even for this period the relationships are considerably different than in the original formula.

The correlations of individual factors with rainfall are:

	−1908 (Walker)		1909–	
Snow	1876	−0.36	1936	−0.09
Mauritius	1875	−.36	1936	−.09
South America	1875	+.42	1936	+.42
Zanzibar	28 years	−.31	1935	+.06

For a few of the factors utilized in these formulae I have been unable to obtain complete or wholly satisfactory data. The most uncertain series is that for snow; although tabulated values up to 1921 are available (1) for the subsequent years I have had to assign numerical values to the worded estimates in the monsoon forecasts published each June. While such matters do not materially affect my verification, they would account for slight discrepancies from those of Walker.

In spite of its early very encouraging performance, the formula has broken down completely in the last 15 years. South America pressure is the only factor in this formula which has survived the test of time.

The two later formulae are for the prediction of the June to September rain in the central and northern part of the India Peninsula. Little or no physical explanation was offered for the new factors involved.

The 1919 Formula is (1):

Peninsula rain = +0. 44 South America pressure, April and May.
　　　　　　　 −.29 Zanzibar rain, May.
　　　　　　　 −.41 Java rain, October to February.

The multiple correlation for 1875–1919 is +0.73. Actual and computed values for 1920–36 are given in table 2, the correlation between these being +0.21. The individual correlations with Peninsula rain are:

	−1919 (Walker)		1920–	
South America	1875	+0.47	1936	+0.47
Zanzibar	1891	−.48	1935	+.14
Java	1880	−.45	1936	−.15

South America is again the only factor to hold good.

The 1924 Formula, for which Walker published plotted values up to 1932 (3), is (6):

Peninsula rain= —0. 22 Cape Town pressure, September to November.
+. 20 South America pressure, April to May.
—. 12 Dutch Harbor temperature, December to April.
—. 24 Java rain, October to February.
—. 32 Zanzibar District rain, May.
—. 26 Southern Rhodesia rain, October to April.

The multiple correlation for 1875–1923 is +0.76. The computed values for 1924–36 are given in table 2; their correlation with actual is +0.12. The individual correlations are:

	Walker			
Cape Town	1875–1923	—0.38	1924–36	—0.06
South America	1875–1923	+.44	1924–36	+.43
Dutch Harbor	36 years	—.38	1920–36	—.33
Java	1880–1923	—.36	1924–36	¹—.08
Zanzibar district	1893–1923	—.42	1924–35	+.32
Southern Rhodesia	1899–1922	—.50	1923–36	—.40

¹ Each coefficient has been computed from the means and standard deviations for the series of the correlation only. Since Java rain was below normal in all of the years 1924–36 except one, and peninsula rain averaged nearly an inch above normal, this particular coefficient may be misleading. For 1924–36, when based on means and standard deviations of the total series up to 1936, it becomes —0.17.

Besides South America, Dutch Harbor and Southern Rhodesia have essentially maintained their original correlation.

TABLE 1.—*Formula of 1908. Departures from normal in inches, India rain, June to September*

	Actual	Calculated		Actual	Calculated
1909	+2.04	+1.50	1923	+1.69	—1.15
1910	+1.69	+1.31	1924	+3.13	+.24
1911	—3.92	+.74	1925	—1.54	+1.04
1912	—1.74	+1.93	1926	+2.62	+1.16
1913	—1.88	—.59	1927	—.80	—.32
1914	+3.40	+.64	1928	—1.51	+.57
1915	—3.00	—1.43	1929	—.27	+2.32
1916	+5.00	+4.15	1930	—.78	—.68
1917	+7.14	+3.43	1931	+1.42	—.43
1918	—6.55	+.51	1932	—1.14	—.63
1919	+3.26	—1.70	1933	+5.10	—.89
1920	—4.31	—1.90	1934	+1.84	—.60
1921	+1.41	+3.40	1935	+.74	—1.47
1922	+2.04	—.39	1936	+1.05	—.15

Considering the three formulae together, eight different factors have been utilized toward predicting the monsoon rain in India. The verification shows that four of these relationships are negligible for recent years, one has reversed its sign, but three have stood up. Thus, in spite of the very slight success attained by the three formulae above, it seems possible that a somewhat more successful one could now be derived.

TABLE 2.—*Departures from normal in inches of India Peninsula rain, June to September*

Actual	Calculated	
	1919 formula	1924 formula
1920 ... —10.9	—1.9	
1921 ... +.1	—1.1	
1922 ... —3.0	+3.3	
1923 ... —.3	+5.8	
1924 ... —2.7	+9.4	+11.0
1925 ... —3.6	+1.5	—2.8
1926 ... +2.4	+9.7	+4.8
1927 ... +2.6	+4.9	+5.9
1928 ... —.1	+1.7	+2.7
1929 ... —3.8	+5.7	—.7
1930 ... —.9	+11.4	+8.5
1931 ... +4.8	+4.0	+1.1
1932 ... +1.1	+1.7	+.9
1933 ... +7.5	+7.7	+5.7
1934 ... +4.4	—.8	+.1
1935 ... +.5	—5.8	—5.2
1936 ... —1.0	+.1	—.8

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REPORT ON THE WORK OF S. K. SAVUR OF INDIA

By R. B. Montgomery

Only two of Savur's papers need be discussed here. In one of these (1) he compares the coefficients used by Walker for the "1924 formulae" with the same coefficients computed from data up to 1930.

The coefficients with Peninsula rain June–September, used in the formula for it, are as follows:

	From—	To 1923	To 1929	5 percent
Cape Town pressure, September to November	1876	−0.38	−0.36	−0.15
South America pressure, April and May	1875	.44	.43	.23
Dutch Harbor temperature, December to April	1882	[1] −.38	−.40	−.19
Java rain, October to February	1880	−.36	−.31	−.08
Zanzibar District rain, May	1893	−.42	−.37	−.11
Southern Rhodesia rain, October to April	1899	[2] −.50	−.45	−.17

[1] 1882–1919.
[2] 1899–1922.

The multiple correlation has fallen from 0.76 for the "1924 formula" to 0.69 for the "1930 formula." He applies a test by Fisher and finds that there is only a 5-percent chance that in the long run the multiple correlation 0.69 will fall as low as 0.45. Also, there is only a 5-percent chance that the individual 1930 coefficients will fall to the values given in the last column of the table above. From this test, which he considers very stringent, Savur concludes that the individual correlations are all significant and that the multiple correlation will fall only "somewhat further." This conclusion is not justified, because Fisher's test applies to coefficients chosen at random, not to coefficients chosen because of their size from a large number.

Similarly the coefficients with northwest India rain June–September are:

	From—	To 1922	To 1929	5 percent
Equatorial pressure	1895	−0.42	−0.35	−0.07
Cape Town pressure, September to November	1876	[1] −.38	−.34	−.12
South America pressure, April and May	1875	[2] .50	.51	.33
Dutch Harbor temperature, March to May	1882	[3] −.42	−.32	−.08
Southern Rhodesia rain, October to April	1899	−.52	−.39	−.10
Snow	1876	[3] −.40	−.34	−.12

[1] 1876–1923.
[2] To 1921.
[3] 1882–1919.

The multiple correlation fell from 0.76 to 0.64 and there is a 5-percent chance that it will fall to 0.36.

The multiple correlation of the "1924 formula" for northeast India rain, June to September, has fallen from 0.52 to 0.39 and there is found to be a chance of at least 5 percent that it will eventually fall to 0, so Savur does not consider it significant.

The multiple correlations for August–September rain in the Peninsula and in northwest India have decreased respectively from 0.66 to 0.64 and from 0.70 to 0.61, with a 5-percent chance that they will decrease further to 0.40 and 0.35. There is at least a 5-percent chance that three of the five individual coefficients in the former will fall to 0 but that none in the latter will.

The coefficients with north India winter precipitation are:

	From—	To 1920	To 1930	5 percent
Seychelles pressure, November to December	1895	0.42	0.39	0.13
Western rain, December	1892	.56	.30	.00
Port Blair rain, December	1876	−.28	−.23	.00
Seychelles and Zanzibar rain	1892	.52	.33	.06

The multiple correlation fell from 0.76 to 0.54 and there is a 5 percent chance that it will fall further to 0.22. Savur believes this formula to be significant also.

The coefficient between Port Blair December rain and northwest India January and February rain was −0.21 for data up to 1913, so this relation has remained very constant, although small. It is strange that the coefficient which has decreased least and which is largest in 1930 is that for Seychelles pressure, which is the only one for which Walker has offered no explanation.

Summarizing the results, Savur says:

Taking the factors as a whole it is not a little surprising that only 7 out of 28 factors have been found by this stringent test likely to become insignificant in the long run, * * *. The selection is a remarkable achievement and is no doubt due, in a large measure, to Walker's intensive study of the influence of a large number of factors on world weather as revealed in the Memoirs published by him from time to time.

It is difficult to gain from this paper a clear impression of the performance of the various factors, because Savur merely compares Walker's correlation coefficients (for the original "selection" series) with the corresponding coefficients for the entire series to date, instead of with the subsequent series alone. In the other paper, discussed below, this defect is remedied for some of the factors.

In the later paper (9) Savur applies the Performance Test, as published by Normand in 1932, (5) to the "1924 formulae." In applying this test the entire data for each factor are divided into two series, the "selection" series covering the years which were used in the selection of factors for the formula, the "test" series covering the subsequent years (including 1933 for the summer formulae and 1934 for the winter one).

The test is applied in either of two methods. According to the first a factor is significant if it has an a priori probability (physical basis), and if the correlation coefficient with the factor forecast for the entire series is not below the level of significance adopted by the India Meteorological Department, namely three times the probable error. According to the second method, in which the factor was originally chosen merely because of a high correlation for the selection series, the test series is considered a random sample; "if the signs (of the correlations for both series) are the same, the corresponding factor has a considerable chance of being significant, the chance increasing with the magnitude of the test" correlation; otherwise it is probably insignificant.

Savur's results are reproduced in tables 1–3. He does not give correlations for both entire and test series, but

only one or the other according as he uses the first or second method.

TABLE 1.—*India Peninsula rain, June to September*

Factor	Selection series r	Test or entire series r	Type of series	Data used	Probable error of r	Conclusion
Cape Town	−0.38	0.02	T	1924–33	0.25	Insignificant.
South America	.44	.40	E	1875–1933	.08	Significant.
Dutch Harbor	−.38	−.27	T	1920–33	.19	Do.
Java	−.36	−.21	T	1924–33	.24	Do.
Zanzibar district	−.42	.39	T	1924–33	.21	Insignificant.
Southern Rhodesia	−.50	−.45	T	1923–33	.19	Significant.

TABLE 2.—*Northwest India rain, June to September*

Factor	Selection series r	Test or entire series r	Type of series	Data used	Probable error of r	Conclusion
Equatorial pressure	−0.42	0.03	T	1923–33	0.23	Insignificant.
Cape Town	−.38	.03	T	1924–33	.25	Do.
South America	.50	.47	E	1875–1933	.07	Significant.
Dutch Harbor	−.42	.27	T	1920–33	.19	Insignificant.
Southern Rhodesia	−.52	1 −.26	T	1923–33	.22	Significant.
Snow	−.40	−.32	E	1876–1933	.09	Do.

1 The published value is +0.26, assumed to be a misprint of sign.

TABLE 3.—*North India winter precipitation*

Factor	Selection series r	Test or entire series r	Type of series	Data used	Probable error of r	Conclusion
Seychelles pressure	0.42	0.36	T	1921–34	0.17	Significant.
Western rain	.56	.34	E	1892–1934	.09	Do.
Port Blair rain	−.28	−.21	E	1876–1934	.09	Insignificant.
Seychelles-Zanzibar rain	.52	−.53	T	1921–34	.15	Do.

The other three formu'ae may be summarized as follows:

Northeast India rain, June to September, both factors insignificant. Peninsula rain, August to September, all five factors significant. Northwest India rain, August to September, all five factors significant.

There are several objections to Sayur's treatment in this later paper:

1. For each of the factors falling under the first method he gives the basis for the a priori probability. In the case of the relation between peninsula rain and South America pressure, this is merely that the Lockyers first noticed an inverse relationship between pressures in the two regions, which suggested a relationship with India rain. But no physical basis for this has ever been advanced. His choice of the first or second method in each case therefore seems quite arbitrary if not incorrect.

2. In the first method he makes no use of the relative values of the correlation found for the selection and test series. It would be well, for instance, to consider the correlation between north India winter precipitation and western rain for the test series 1921–34.

3. Under the second method he calls the test series a random sample. But, if the correlations for the selection and test series merely agree in sign, the factor is called significant—an obvious contradiction. As an example, the correlation between peninsula rain, June to September, and Java for the test period is less than its probable error, hence it could not justly be considered significant. For the series 1924–36, I have found this coefficient to be only −0.08.

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POLAR ICE AS A FACTOR IN SEASONAL WEATHER

By I. I. Schell

CONTENTS

I. INTRODUCTION

PREFACE

The number of authors who have studied possible relationships between polar ice and weather is considerable. I therefore thought it best to consider the various papers on this subject not according to their origin but rather according to the type of relationship that is involved. Because, in the main, the relationships appear to form a somewhat unified picture, an attempt was made to treat these as a whole. With the exception of a few attempts which I made to clarify certain results, the discussion in sections A and B and some remarks at the end, the contents of this report represent only those contributions which I studied.

GENERAL DISCUSSION

It is held (1, 2) that an increase or decrease in the amount of ice in polar regions, in addition to affecting the weather in the ice area, also has a marked effect on the weather of the regions adjoining the ice area and, less directly, also on the weather of more distant regions. The general effect to be expected from an increase in the amount of ice is a lowering of temperature of the overlying air and also of that of adjoining regions, because of the mobility of the air. A study of continentality made by Brooks (1) indicates that there is an appreciable cooling at least 500 miles away from the edge of the ice. Thus, as the amount of ice in polar regions increases the zone of lowered temperature is also displaced equatorward. Corresponding to the fall in temperature a rise in pressure should occur.

It is held by Wiese (2) that, because of the drag exerted by moving ice on water, an extended and continuous drift of ice from polar to lower latitudes is responsible for a large amount of cold but relatively nonsaline and hence light water reaching the lower latitudes. Further, according to Helland-Hansen and Nansen (3, p. 356) "the ice will, by melting, form a surface layer of light and cold water which will prevent contact between the ice and the warm saline water." Again, when the ice melts, a large amount of cold water is liberated and the surface of the ocean is cooled. Since the added water is considerably lighter than the saline ocean water, there is thus formed a relatively stable stratification. In other words, the effect of the ice, of the cold polar water which the ice brings with it, and of the cold water which forms as the ice melts tends to persist. As a consequence the pressure over the colder surface becomes and tends to remain relatively high; the pressure distribution is thereby changed, which, in turn, must affect to some extent the distribution of pressure over more distant regions. Moreover, some of the cold water is carried by winds and ocean currents to lower latitudes so that the effects described above probably extend over a large area. It is held (4) that the large temperature differences set up by the driving of cold polar water into warm ocean currents favor the development of cyclonic depressions. Consequently the lows should tend to change their mean course in response to the equatorward displacement of the mixing zone of cold and warm waters.

In addition to the "direct" role played by ice in the weather, it is held by Wiese (5, 6) that the state of ice in polar regions reflects the intensity of the general atmospheric circulation and therefore can serve as an indication of the weather throughout the world. Wiese's thesis that the amount of ice reflects the intensity of the general circulation is based on the hypothesis that a large amount of ice in polar regions is associated with a weakening of the general circulation. Presumably, as the intensity of the atmospheric circulation decreases, the transport of warm air to polar regions from lower latitudes diminishes. In addition to the direct loss of heat, the effect of a diminished transport of warm, moist air is a marked loss of heat through radiation from the snow surface due to a lesser amount of water vapor and clouds present in the atmosphere. The result is a lowering of temperature and an increase in the amount of ice. This will be followed by a general rise of pressure in the polar regions. The opposite condition will be expected to arise with an increased circulation.

In line with the above assumption of a decreased intensity of the general circulation there is to be expected a fall of pressure in the high-pressure cells of middle latitudes and thus a general weakening of barometric gradients. One might expect, therefore, the pressure difference between the Icelandic low and the Azores high to be smaller with a weakened circulation, or, presumably, with a large amount of ice in polar regions. Similarly, because of diminished general convection in equatorial regions, there would be expected a decrease of rainfall in low latitudes, although this may be offset by an increase in local convective showers.

In considering polar ice as a possible factor in the weather it was thought desirable to treat separately the relationships between ice and the weather in its immediate vicinity and the relationships involving the world weather at large.

In addition to the relationships which presumably have some sort of a physical basis, there will be treated relationships which, because of statistical considerations, might

be expected to have a certain reality. These relationships are between ice and Walker's oscillations. For definition of oscillations see Walker's papers listed in the bibliography and especially the "Report on the work of G. T. Walker" by R. B. Montgomery, in this volume.

Finally there will be considered relationships between ice and meteorological elements which involve no hypothesis as to cause and effect.

Further it appeared that there are two kinds of ice in the oceans, sea ice and land ice (icebergs). Their significance in relation to weather is different and therefore they will have to be treated separately. However, very few relationships involving icebergs have been studied, and since no physical basis appears to have been claimed for these relationships they will be treated together.

Accordingly, there will be surveyed in this report relationships between sea ice and (A) the weather of adjacent regions, (B) world weather in accordance with Wiese's hypothesis and Walker's oscillations, (C) meteorological elements without reference to any particular scheme, and finally (D) relationships between icebergs and weather.

CHARACTER OF THE ICE DATA

It appears from a number of investigations that polar ice is a very complex phenomenon and at present is little understood (7). This is to some extent due to the rather limited and apparently meagre ice observations. It may be said however that the ice, in addition to the seasonal variation, also frequently undergoes large variations from one year to another. "Ice" in polar regions refers to the large or small bodies of floating ice originating on the ocean surface. "Icebergs," which are also found floating in the oceans, refer to fresh water or land (glacier) ice. The two must be considered separately.

Sea ice.—The main body of sea ice in the northern hemisphere is the thick "compact" sheet ice found in the Arctic Ocean; in the Southern Hemisphere, it is the sheet ice in the seas bordering the ice and snow covered Antarctic continent. Next to the "solid" ice pack, going equatorward, there lies a zone, in some places hundreds of miles wide, which is covered by "solid" ice during only the cold season and by more or less broken ice during the warm season. This zone, called the fringe of the pack, shrinks in the late summer and fall and widens in winter and spring. In the latter period it reaches, in the northern hemisphere, to the northern coast of Asia, and several hundred miles into the open Atlantic and Pacific.

Another source of ice is the coastal regions in the polar latitudes. In the shallow bays and inlets and along the shallow but often very wide stretch of coastal waters the water, in the cold season, often freezes clear to the bottom, thus becoming fast to the shore. The ice then proceeds to grow out into the sea for a considerable distance, in some cases hundreds of miles (North Siberian Shelf), although the portion attached to the bottom is probably generally confined to a narrow belt near the shore. With the approach of summer, the outer portion is the first to break off and drift away. Next the ice extending from the surface to the bottom separates from the shore and the ocean floor, drifting away. It often happens, however, that the fast ice persists through the summer. Then open water can be observed far out in the sea while the coast and bays remain ice bound.

The thickness of the ice varies considerably. The semipermanent character of the polar ice pack indicates intensive growth of ice and a thickness of several meters. The ice that forms in the open seas at lower latitudes, where it persists only during the cold season, is relatively

thin, its vertical growth depending on the shallowness of the ocean over which it forms. Over the North Siberian Shelf, for example, the ice is usually very thick while the ice forming over Greenland Sea is usually less than a meter deep.

An important factor in the thickness of sea ice and also a far-reaching factor in the ice distribution is the almost continuous movement of the ice. The movement is determined by forces which vary with the particular location and with the general circulation of the hydrosphere and atmosphere. Thus various portions of the pack are subject to different forces so that their direction and movement must vary too. As a result, there is a continual rearrangement of ice taking place within the pack, but mainly along its fringe, where the forces acting are more intensive and divergent. This is accompanied by
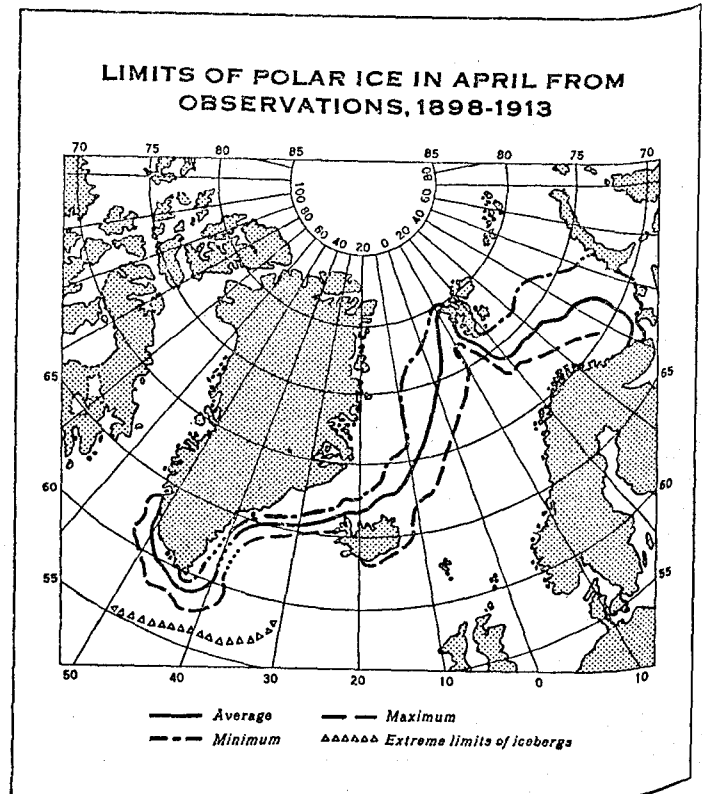
FIGURE 1.—Reproduced from Shaw's Manual of Meteorology.

numerous breaks in the "solid" ice and the formation of areas of open water. At the same time, pieces of ice are heaped up one on top of another.

It can be seen that the effect of these forces is also to cause ice from the fringe of the pack to be driven to lower latitudes and for ice from lower latitudes to be driven into the pack. Thus there is a continual replacement of ice along the fringe. The process, however, is not uniform. The feeding of ice into the pack takes place mainly in the early part of the cold season while the disgorging occurs mainly in the warm part of the year.

In addition to the varying movement of individual portions of the pack there is a general slow drift of the whole pack from east to west. In the North Polar Basin the entire pack appears to move from Bering Strait toward Greenland Sea which is the main outlet for north polar ice into warm waters. Thus the pack continually renews itself and it is estimated that the average life of the ice, from this pack is less than 5 years. In some regions, however, the ice may attain a much greater age.

The sea ice that is met with in the lower latitudes of the polar regions is therefore generally composed of fragments from the heavy polar pack (old thick ice) that are carried down by wind and ocean currents, relatively thin (young) ice which forms locally in the winter and spring along the outer edge of the fringe, fast ice of varying thickness leaving the shore, and finally ice of all these types which has drifted (in some regions) from the neighbouring seas.

Variation in ice conditions.—In addition to the large seasonal variation in amount of sea ice, as observed from the position of the "fringe" of the ice pack, and from ice lying offshore, there is also a large, irregular year-to-year fluctuation. See fig. 1 reproduced from (54). Thus in Barents Sea, this variation amounts to hundreds of thousands of square kilometers. In the table below (reproduced from Wiese (8)) are given the maximum and minimum areas occupied by ice over a period of about 20 years.

Extreme values of area covered by ice in Barents Sea (1895–1915) in 1,000 square kilometers

	April	May	June	July	August
Maximum	1364	1296	1225	708	499
Year	1899	1907	1903	1913	1912
Minimum	949	712	553	248	170
Year	1898	1897	1898	1908	1901, 1908

The variation in Greenland Sea, though proportionately less, is actually larger, for the total area involved is greater. The maximum variation in the total area is estimated to be 800,000 square kilometers. Again, in some years Iceland has been known to be completely surrounded by ice in winter and spring except for a portion of its west coast, while in others only a little ice was observed off its northern shores.

Unlike the case of temperature or of pressure no accurate and uniform index has been as yet developed for the determination of ice conditions.[1] The lack of an accurate index is due largely to the impossibility of observing the entire or even a large part of the ice-covered area of the polar seas. Until recently, in the Northern Hemisphere, only certain parts of the fringe of the Arctic pack had been studied. But even here the ice-covered area was estimated mostly from observations at the outer fringe. Only of late have better observations become available. These are obtained by relatively frequent airplane flights from Northern Siberia. In the Southern Hemisphere, the observations are more meagre and inadequate. With the exception of the South Orkneys only very fragmentary information exists about ice conditions in the Antarctic regions.

The lack of uniformity of the index of ice is due to the varying character of the observations. These can be divided into two distinct types, one of ice observed from the shores of the islands or the mainland off the coast of which it lies, the other of ice observed by ships plying in the polar seas. The first type refers to ice conditions over a comparatively small area, "as far as the eye can see," from a fixed point. The second type of observations generally refers to the position of the boundary of the large ice mass which often may bar the persistent efforts of ships to penetrate farther into the polar regions. For this reason, and also because the ice regime near land often differs considerably from that in a large sea, it was thought best to consider the two types separately.

[1] The thickness of the ice is not taken into consideration. The above term refers mainly to the area of ice-covered ocean surface or else to the persistence of the ice at a given place.

Ice observed offshore.—The islands from which observations have been considered are Iceland, Spitzbergen, and the South Orkneys. The latter lie about 600 miles due southeast of the southern tip of South America.

The Iceland observations date back to the thirteenth century. The early records were meagre and appeared in the sagas and written literature of the day, but as time went on they became more complete and systematically compiled. Records of ascertained ice conditions at Iceland from 1233 to 1877 were compiled by the Icelandic traveler T. Thorodssen (9), but for the purpose of investigation only those beginning with the end of the eighteenth century were thought to be sufficiently extensive and trustworthy to warrant their use. From 1877, due to better organization in the collecting of the observations, they became more extensive and reliable. The ice conditions were classified according to the length and character of the ice period, whereby a day with "heavy" ice conditions was given double weight. This index of duration and intensity was designed by Meinardus (10) in 1906 and has been accepted and employed by others since. Meinardus made tabulations for periods of 1 year but since then monthly periods also have been computed. It was found that ice occurs most frequently during April and May, and least frequently from September to December (only once was ice reported in October during the nineteenth century). Most often the ice begins to appear between January and March and it lasts on the average 2 months and 20 days. Since the earlier it appears the longer it lasts, it is evident that the period of heaviest ice falls in the spring. The causes of this annual distribution were investigated by Meinardus (10) who pointed out that the quantity of ice in the east Greenland current is least in autumn, when the ice is so near the Greenland coast that Iceland is almost invariably free of ice. The beginning of the ice season at Iceland is determined by the increase of the ice in the Denmark Strait which occurs late in the autumn. If the increase is great enough the strait becomes filled with ice and it begins to appear off Iceland. Ice is also brought down, especially along the eastern shore, by the east Icelandic current, which runs in the southern part of the Greenland Sea approximately between latitudes 67°–71° N. In extreme years by far the greatest portion of the Icelandic coast is surrounded by ice. In some years, however, the amount of ice is insufficient to fill the strait and Iceland remains quite free of it. (See fig. 1.)

Unlike Iceland, the group of islands forming Spitzbergen experiences an abundance of ice almost all the year round. This is not true, however, of the northern half of the west coast, which generally remains free a considerable portion of the year. The dominant factor in the ice distribution is the warm current moving northward along the west coast and the cold, polar, ice-bearing current moving southward along the east coast. The latter current carries ice also along the south coast and, like the east Greenland current, often causes ice to round South Cape and be discharged into the warm current thus to be carried by this current northward along the west coast. The average distribution of ice off the coast of Spitzbergen for the period 1898–1921 was summarized by Frommeyer (11). Only the 5 warm months, April to August, were considered. The ice character is given by the area of ice in Spitzbergen waters, between longitudes 10°–20° E. and north of latitude 75°.

The other fixed region figuring in the investigations is in the Southern Hemisphere, the South Orkneys. The observations covering this region date back only to 1902.

The index of ice conditions was designed by Mossman (12). The character of the ice is defined as "open" or "close" and sometimes as "very close." The observations were made every day from an elevated point on land and the summary of ice conditions for each season refers to the general conditions of the seas surrounding the South Orkneys. The ice appears to have its source to the South in Weddell Sea and it persists, *at the islands,* on the average, for about 8 months of the year.

Ice observed at sea.—The second type of observations concern the position of the outer fringe of the ice pack. The mean position of the edge of the pack is computed, for the Northern Hemisphere, for each month of the ice season, from reports sent in by ships. From the position of the boundary, the ice-covered area in units of 1,000 square kilometers can be determined by planimetrical measurement (13). It is thus assumed that poleward from the boundary no large areas of open water exist, an assumption which probably is not true, in some sections at least. The task of collecting and publishing the Arctic ice data was assumed by the Danish Meteorological Institute in 1899, but the records published by this institution go back to 1894. The information is published annually in the "Isforholdene i det Arktiske Have" (the state of ice in the Arctic Seas). It is given in the form of a brief summary of the prevailing conditions during each year, supplemented by individual monthly summaries for the various regions. These are interspersed with many actual reports.

The earliest systematic, continuous, and fairly reliable data available are for Greenland Sea. The region is defined by the Danish Meteorlogical Institute as "the area between the meridian of Cape Farewell, the east coast of Greenland, 80° N., and the meridian of South Cape." These data go back to 1877. From that year to 1892 they were compiled by C. Ryder (14). The position of the boundary is represented cartographically. The ice season in Greenland Sea is generally from April to August inclusive, but pack ice is seldom absent from the waters of northeast Greenland and the Greenland Sea. The ice, during the season, is mainly composed of fragments of the old heavy pack that have arrived directly from the Arctic Ocean, plus young ice formed in the winter and spring in Greenland Sea proper, and ice that has drifted westward from Barents Sea. The influx of heavy ice occurs in early winter. This ice, reinforced by great quantities of young ice forming locally, spreads gradually southward and eastward as a large mass. The most southerly position of the ice boundary is generally attained late in the spring. With the approach of summer the boundary begins to recede, retreating to its most northerly position in the autumn.

Systematically recorded and published observations for Barents Sea date back to 1895. The region is defined (13) as "the area bounded by the meridian going through South Cape, the coast of Spitzbergen, latitude 80° N., meridian 70° E., and the west coast of Novaya Zemlya." The ice season is generally from April to August. Most of the ice in this region is of local origin, which in part explains its great seasonal and annual variation. The other ice met with here is brought in from the north polar pack by the current flowing southward along the east coast of Spitzbergen, and from the neighboring Kara Sea.

The observations in the Kara Sea date from 1869. However, the number of years for which more or less complete records are avilable is small, but 21 years, during the period ending with 1928. The region is defined as the area bounded by the east coast of Novaya Zemlya and the 70° E. meridian. It is generally almost entirely frozen over until July, and only in August and September is the variation from year to year sufficient to provide figures suitable for consideration. Several factors exist which make the ice persist for a long time and grow very thick.

The Arctic fringe.—In addition to the relationships involving ice conditions in individual regions an attempt was made to trace a relationship using ice in the Arctic taken as a whole. Practically no observations of the Arctic pack exist. It was thought however, that since the fringe of the pack covers several regions where ice conditions have been observed, a very rough estimate of the extent of ice in the Arctic could be obtained from ice conditions in the former. Accordingly an index was designed for ice in the Arctic which is given by the following formula (4):

5 (Greenland Sea ice, April to August) + 7 (Barents Sea ice, April to August) + 2 [3(Kara Sea ice, August) + Kara Sea ice, April to August].

Only the data from the Greenland, Barents, and Kara Seas were employed. With the exception of Iceland any consideration of other regions was limited by lack of data. In the formula, less weight was assigned to Greenland Sea than to Barents Sea region. The reason offered was that the former region, being a far outpost of the Arctic pack, is probably less representative than the latter region. Apparently for a similar reason Iceland and other regions were entirely left out of consideration. The values of the "ice index" for the Arctic were computed beginning with the year 1895. It must be noted in connection with the formula, that the annual variations in ice in the above regions often differ considerably one from another. For example, no correlation was found between ice of the Greenland Sea and that of the Barents Sea. There is also some evidence that when ice is plentiful in the North Atlantic region it is relatively scarce off the northeast Siberian coast.

The last two regions to be considered are in the Western Atlantic; namely, Davis Strait and the region south of Newfoundland.

Davis Strait.—There are two distinct types of ice in Davis Strait, "west ice" and "storis." "West ice" is that which comes from the Baffin Bay region and from the North sounds and bays along the eastern shores of the North American continent, and the ice which is formed locally in the strait itself. "Storis" is ice which drifts northward from Cape Farewell along the west coast of Greenland. In the height of the season the "west ice" stretches far eastward, while the "storis" forms a belt of many miles in width. Under certain conditions the "west ice" joins with the "storis" and the wide area of open water which normally separates them disappears. Since the variation in conditions of "west ice" was not considered in connection with the study of the pressure distribution, the brief description below is limited to "storis" alone; and hereafter, by the variation in ice conditions in Davis Strait will be meant the variation in "storis" only. This ice is composed mainly of fragments of the North Polar Basin pack, carried southward by the east Greenland current to Cape Farewell and thence round it and northward by the warm current into Davis Strait. The "storis" forms a belt varying in width and compactness. It may sometimes attain a breadth of 200 miles, and again be extremely narrow, only several miles. Its arrival at Cape Farewell occurs normally in the middle of January. Julienhaab Bay to the north is filled in February while Fiskenaes, farther up, is reached in April. The farthest north usually reached by the ice is Fiskenaes, more rarely

Godthaab, and very rarely Sukkertoppen. With much ice along the east coast of Greenland it appears at Cape Farewell earlier; then it also occurs in greater abundance in Davis Strait. The greatest amount of ice is found in May, perhaps also in June; in July the ice decreases and in August it sometimes completely disappears from the west coast of Greenland. The observations employed in the study referred to above date back to 1895. An index of ice conditions in this region on a scale of 0–10 was devised by Speerschneider (15).

South of Newfoundland.—Davis Strait serves as an outlet to the North Atlantic for the ice from Baffin Bay and the Arctic sounds, to the westward. To this stream of ice there is added, on the way southward, the discharge from Hudson Bay, Fox Basin, and other western channels. In the vicinity of Labrador the ice keeps close to the coast, and eventually spreads out past Newfoundland, where it usually lasts from February to May. During the height of the season, March and April, the ice sometimes extends as far south as the 40th parallel but on the whole it remains within the limits of the shelf waters. The variation in ice from year to year is sometimes very great. Years with practically no ice have been observed and others when ice was very abundant.

The observations employed in the studies of relationships with Newfoundland ice date from 1860. The older records were compiled from reports of ice sighted by trans-Atlantic ships on their regular crossings through the ice regions off Newfoundland, and were published by several institutions such as the United States Weather Bureau, the Hydrographic Office in London, and others. Since 1913 the observations have been compiled by the International Ice Patrol whose special task is to observe, study, and forecast the drift of ice. From the various observations the position of the ice boundary was determined, usually for each month of the season.

Three or more separate indices were used in the studies involving Newfoundland ice. Meinardus (16) for the period 1860–1902 used the intensity of ice given on a scale of −2 to +2, and after 1880 with ½ unit steps. Schott (17) considered the position of ice boundary usually for each month of the season as determined from various observations for the period 1880–91. More recently, beginning with 1913, Smith (18) has employed a scale of 0–10.

In closing the preliminary discussion of the observations and character of sea-ice conditions it should be noted that the authors of the investigations surveyed below made an attempt to limit themselves to years which were characterized by a large variation in the ice conditions, and years for which relatively complete information regarding these conditions existed.

Icebergs.—It is estimated that only an extremely small portion of the ocean ice is composed of land ice, or icebergs, most of which originate in Greenland and on the Antarctic Continent.

The chief berg-producing regions of Greenland lie near latitude 70° and south of it. The majority of the icebergs that finally reach the open ocean are, however, from the west side of Greenland. The bergs from the east coast are generally carried by the east Greenland current southward to Cape Farewell then around it and northward. Few of these ever get into the Labrador current and the open Atlantic. A few of the icebergs from east Greenland may be caught in the east Icelandic current and then carried far southeastward. The bergs which have been observed near the Faroes probably have followed that route. The icebergs that come down with

the Labrador current to the offing of southern Newfoundland, the ones which concern us in this report, originate mostly on the west coast of Greenland. Relatively few are produced by the glaciers on Baffin Land and elsewhere. The actual number of bergs carried to the Newfoundland region (48th parallel) is observed by the ice patrol, and a table of these numbers beginning with year 1900 has been published by the United States Coast Guard (19). The season for icebergs is from the middle of March to the middle of July, with the maximum occurring in May.

There is very little information about the iceberg regime of the Southern Hemisphere. The London Meteorological Office has been publishing the number of bergs separately for each of the three oceans which comprise the Southern Ocean (Indian, South Pacific, South Atlantic) since 1885. Earlier records are contained mainly in two papers by H. C. Russell (20). One of the heavy ice-producing regions is the Ross Barrier in the southern fringe of the Ross Sea, another is in the Weddell Sea. It would appear that there are other numerous berg-producing regions on the Antarctic Continent, but since a great portion of this land is unexplored there is little known about the place of origin of many of the bergs. However, the icebergs caught in the pack are freed for the most part when they reach the Ross and Weddell Seas. Unlike the Northern Hemisphere where they are generally confined to a rather limited area and to a more or less definite season, the Antarctic bergs have been observed in almost every longitude of the South Ocean, and in every month of the year, although most frequently and in greatest numbers in the South Atlantic. They are naturally far more numerous and often much larger than in the Northern Hemisphere, and generally drift from west to east (in the open ocean) advancing in large numbers, often more than a hundred at a time.

Icebergs and pack ice.—While in the main sea ice and land ice were employed separately in the various investigations, in a few cases icebergs were treated together with the pack ice appearing in the same region. The question that arises is whether there is any relationship between the two types of ice, and to what extent the presence of one is an indication of the presence of the other.

In the regions of origin of ocean ice the shape of the coasts and general distribution of land will exert a strong influence on the movement and direction of the floating ice, sometimes causing all the ice to follow a single course. In the relatively open seas, wide straits and sounds, icebergs are carried mainly by the current, while the drift of pack ice is affected greatly by the wind. In such a case the one will move more slowly than the other and when the direction of the current is different from that of the wind the icebergs will move "away" from the pack ice or pack.[2]

The mutual relationship between pack ice and icebergs insofar as it concerns the investigations with which we are concerned appears to be as follows. In the case where there is an abundance of pack ice along the coast, its presence will tend to prevent the bergs from leaving the region of their origin, even though the pack itself is drifting. Thus the relative smallness of the number of icebergs that come down from the east coast of Greenland is due to some extent to the heavy pack ice drifting southward along the coast where grounding in the shallow bays is very important, with the result that the heavier the pack or the farther the extent of sea ice, the fewer the

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[2] "The sight of bergs moving in an apparently different direction from the main pack is a common sight. In some cases there is an actual difference in direction." ((21), page 375.)

icebergs observed. Conversely, the absence of pack ice may be associated with a large number of icebergs. Thus R. C. Mossman states ((22), p. 410) "the absence of the pack in the Weddell Sea area (and the high temperature) were in some way associated with the extraordinary accession of icebergs reported last winter (1908) in the region of Cape Horn and in the South Atlantic as far north as the 40° of south latitude." In some cases, however, an abundance of pack ice means also an abundance of icebergs. Thus 1906 is regarded as a heavy ice year, from the standpoint of both icebergs and pack ice in more or less the same regions. Also E. H. Smith (18) found for the period 1880–1924 a correlation coefficient of 0.87 between the number of icebergs and amount of pack ice south of Newfoundland.

The presence of ice in bays and inlets and along the coast prevents the icebergs floating on the outside edge of the pack from stranding. Thus a large amount of pack ice along the Labrador coast means a large number of icebergs. The above discussion allows the conclusion that the relation between icebergs and pack ice depends on the local conditions.

II. RELATIONS BETWEEN POLAR ICE AND METEOROLOGICAL DATA

Introductory remarks.—Before presenting the results of the investigations it must be reiterated that in accordance with our plan there will be considered in this section of the report only relationships between the Arctic and Antarctic ice and the meteorological elements in the adjacent regions. Relationships involving more distant regions will be treated in the remaining sections.

Brennecke (23) and Meinardus (10) found early in the century, that years characterized by heavy ice conditions off Iceland and in the Greenland Sea in general, tend to be accompanied by relatively high pressure in the vicinity of Iceland and Greenland and relatively low pressure over the Norwegian Sea and northern Norway. Conversely, years characterized by light ice conditions tend to be accompanied in the same regions by low and high pressure respectively. More recently Wiese (2), Brooks (24), and others, using more extensive data, have found practically the same relationship. It was indicated from the material employed by Meinardus that also the subsequent pressure distribution is associated with ice conditions but it fell upon Wiese actually to show that this connection is apparently real. The search for relationships between the pressure distribution and ice conditions was extended to include on the one hand other regions abounding in ice, and on the other hand, the pressure over wider areas.

A. Sea ice and the weather of adjacent regions

1. NORTHERN HEMISPHERE

Pressure.—The earlier efforts were generally limited to the study of the pressure departures at a few points and to the variation in pressure differences between some of these points, in relation to the variation in ice conditions. Thus Brennecke, in 1904 (23), gave the spring pressure departures (from the 20-year mean) for a number of points in the North Atlantic, for each of the 4 exceptionally heavy and the 2 exceptionally light ice years in the Greenland Sea region which occurred within the period 1877–95. These years were: exceptionally heavy, E+, 1881, 1882, 1888, 1891; exceptionally light, E—, 1884, 1889. The mean pressure values and departures were obtained for each of the points given by the intersection

of the 10° longitude and the 5° latitude circles. In addition the differences in pressure between the two points at 20° W. and at 20° E. along latitude 70° N., were given for each year of the entire period except 1883. This year was left out because of insufficient data.

An examination of the pressure departures for the area bounded by latitude 60° and latitude 70° N. and by longitude 10° and longitude 30° W. (Iceland region) shows the pressure in 1882, and to some extent also in 1881, to have been below normal, while in the other 2 exceptionally heavy ice years it was above normal. For the 2 light years, the pressure was considerably below normal, more than in the case of 1882. For the area bounded by the

I = Pressure difference between 20°W and 20°E long. at 70° N lat.
II = Air temperature, departure from normal, Stykkisholm
III = Sea surface temperature, departure from normal, Papey

++ Very heavy ice   + Ice somewhat above normal
  - Ice somewhat below normal   -- Very light ice
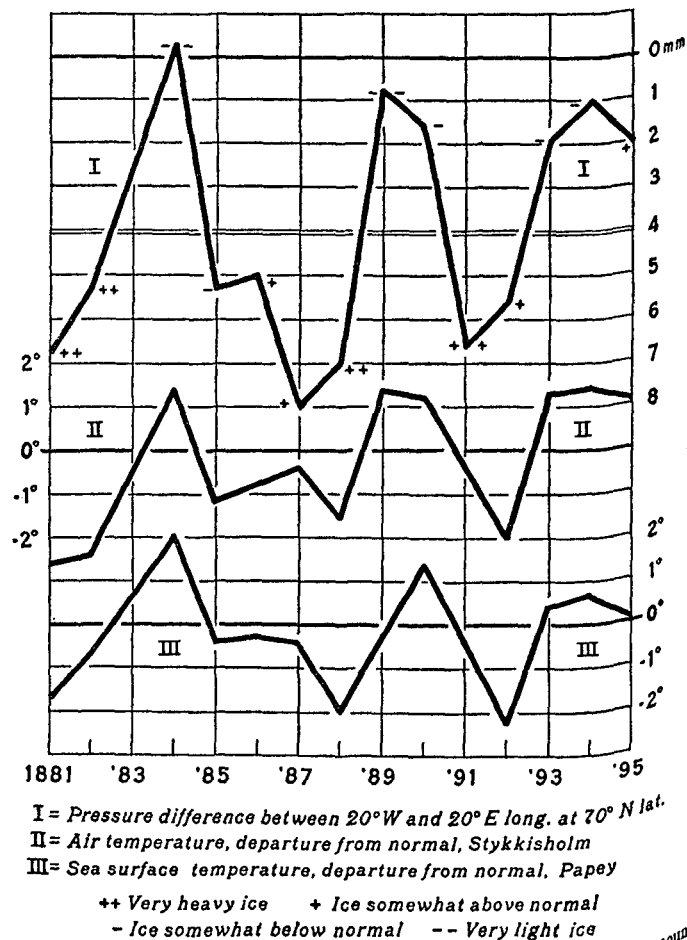
FIGURE 2.—Relation of pressure and temperature variations, March to May, to amount of ice. Reproduced from Brennecke (23).

Norwegian coast, by latitude 60° and latitude 70° N. and by the Greenwich meridian (Norwegian Sea region) the pressure was below normal in every one of the four heavy years, and slightly above normal in the 2 light years. More significant is the trend in the pressure difference between the westerly and easterly points on lat. 70° N. (Greenland and Northern Scandinavia) during these and the other years. An inspection of the diagram (see fig. 2) shows that the pressure differences during the exceptionally heavy and during the exceptionally light years were, respectively, considerably above and considerably below the normal value between these points, which is 4.1 millimeters. The average for the four E+ years was 6.4 millimeters, and for the two E— years nearly zero. With the exception of 1885 and 1895 the same trends are shown also for the years characterized by only moderately abnormal ice conditions.

The above results (see table 1) show that though the pressure in the Icelandic region is not always "high" when heavy ice conditions prevail there it is nearly always higher relative to the normal than is the pressure off the Norwegian coast.

TABLE 1.—*Departures from normal of pressure in millimeters at 20° W. and 70° N., Alten and Bodö, and departure differences between 20° W., 70° N., and Alten and Bodö, respectively*

Years	20° W., 70° N.	Alten— 23° E., 70° N.	Bodö— 14° E., 67° N.	20° W., 70° N.— Alten	20° W., 70° N.— Bodö
Heavy, E+					
1881	-0.6	-4.1	-2.3	3.5	1.7
1882	-2.2	-4.2	-3.2	2.0	1.0
1888	2.4	-1.1	-1.4	3.5	3.8
1891	1.4	-2.1	-1.6	3.5	3.0
Light, E—					
1884	-1.9	2.1	1.9	-4.0	-3.8
1889	-1.6	.9	.6	-2.5	-2.2
Mean pressure, 20 years	761.6	758.2	758.3		
Mean pressure difference, 20 years				3.4	3.3
Average pressure difference, E+				6.5	5.7
Average pressure difference, E—				-.6	-.5

Despite the negative departures in 1881 and 1882 at 20° W., 70° N., which were apparently produced by the retreat of the Greenland anticyclone northwestward (see fig. 3), there is still maintained, by virtue of an intense development of the Norwegian low, an abnormally large pressure difference. We thus may look upon the years 1881 and 1882 as characterized by a general deficiency of pressure over the entire region. The relationship between ice conditions and pressure difference is not impaired thereby.

In 1922, Wiese (2) published maps of the mean pressure distribution for the North Atlantic and Europe for the spring, summer, and fall seasons, averaged separately for 7 heavy and 4 light ice years in the Greenland Sea region. The period treated was 1880-1916. The pressure maps utilized in the study were in part Hoffmeyer's maps and in part maps constructed from available data at the Central Geophysical Observatory at Leningrad. The years 1880 and 1911 were not included because Hoffmeyer's maps were not available to Wiese. Further, because of scarcity and unreliability of ice observations during 1886, 1889, 1893, 1894, these years also were omitted from consideration. The years selected were:
E+ 1881, 1882, 1887, 1891, 1895, 1896, 1906.
E— 1897, 1899, 1904, 1908.

The character of the year was determined from the ice conditions between latitudes 67° and 71° N. Two of the reasons given by Wiese for this choice were (1) that the observations from farther north are scarce; and (2) that the east Iceland current branches away from the heavy ice-bearing east Greenland current south of 71° N. so that the area chosen represents best the ice transport of the parent current which, in turn, is quite probably a good indicator of ice conditions in Greenland Sea.

The spring results appear to be quite consistent with Brennecke's findings (see table 2) which were based on a shorter period of years, even though, by virtue of the difference in criteria for ice conditions employed by the two authors different years were sometimes used by them. Thus 1884, 1888, and 1889 were not used by Wiese, who considered them only moderately, not exceptionally, abnormal. For the sake of comparison I present in table 2 the pressure difference obtained by Wiese between the points already treated by Brennecke.

TABLE 2.—*Spring pressure difference in millimeters between 20° W., 70° N., and Alten and Bodö*

	Brennecke (1877-95)	Wiese (1880-1916)
E+	*4 years*	*7 years*
20° W., 70° N.—Alten	6.5	4.5
20° W., 70° N.—Bodö	5.7	4.0
E—	*2 years*	*4 years*
20° W., 70° N.—Alten	-0.6	0.5
20° W., 70° N.—Bodö	-.5	.5

The agreement is reasonable and bears out the fact that when heavy ice conditions prevail in the Greenland Sea region the pressure gradient from Iceland eastward is steeper than normal.

In 1906, Meinardus (10) presented the monthly departures from the normal, as well as the mean pressure differences between Stykkisholm (west Iceland) and Vardo (north Norway) for heavy, E+, and light, E—, ice years off Iceland. The values were based on a period of 35 years (1866-1900) during which time 7 years of the E+ type occurred and 6 of the E— type. He found (see table 3) that the value of the pressure difference, which was normally negative, was smaller for the E+ years, from October to July, inclusive, but somewhat greater the remaining 4 months. In some months, March to May, the pressure difference actually became positive. For the E— group the negative pressure difference became larger for every month except July but the increase was not as large as the decrease in the other case. From an examination of the differences between the departures (last line of table) it appears that a consistent relationship between ice conditions and Stykkisholm-Vardo pressure difference is maintained from October till July. Its essence is that heavy ice conditions at Iceland, generally from January to July, are preceded from 1 to 2 months by relatively high pressure at Stykkisholm and low pressure at Vardo, generally from October to June and, conversely, by low pressure and high pressure, respectively, when the ice season is light.

TABLE 3.—*Pressure difference by months Stykkisholm-Vardo and pressure departures from the mean in heavy ice (E+) and light ice (E—) years*

PRESSURE DIFFERENCE (1866-1900)

	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI
E+	-0.5	0.0	-1.0	-0.9	-0.1	0.9	1.6	1.3	0.0	-1.1	-2.2	-3.2	-2.5	-3.2
E—	-2.2	-3.5	-5.9	-5.8	-3.2	-1.7	-2.1	-2.8	-2.0	-.9	-1.9	-1.9	-1.3	-3.5

DEPARTURE FROM MEAN PRESSURE DIFFERENCE (1866-1900)

	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI
E+	0.7	2.0	2.9	3.6	2.9	2.1	2.4	2.4	1.6	0.6	-0.7	-1.7	-1.3	-1.2
E—	-1.0	-1.5	-2.0	-1.3	-.2	-.5	-1.3	-1.7	-.4	.8	-.4	-.4	-.1	-1.5
Difference	1.7	3.5	4.9	4.9	3.1	2.6	3.7	4.1	2.0	-.2	-.3	-1.3	-1.2	.3

In 1923 Brooks (24) using the period 1901-19 found the average pressure at Stykkisholm, in the majority of the 43 months [3] during which ice lay off Iceland, 2 mb. above the normal for the corresponding months. Taking the actual days during which ice was reported and confining oneself to periods of more than 5 consecutive days the mean deviation during the whole of the 701 ice days thus obtained was 6.7 mb. This result probably means that when the Stykkisholm-Vardo pressure difference diminishes it is generally due at least in part to a rise at

[3] I could not learn from his article the extent of the majority and what those months were, but, from our knowledge of the ice season, they probably occurred during the winter and spring.

PRESSURE DISTRIBUTION IN YEARS OF LIGHT AND
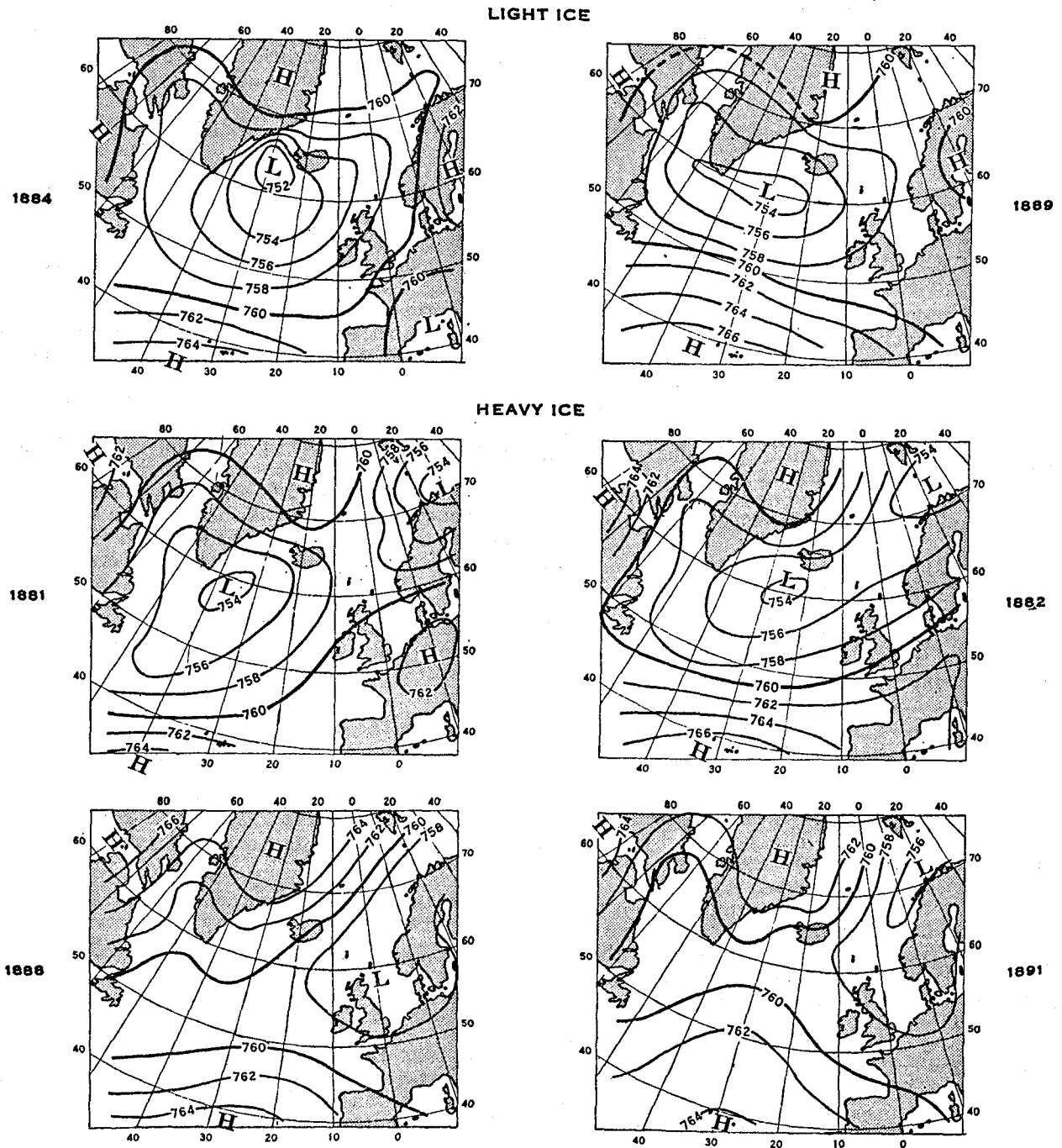HEAVY ICE IN GREENLAND SEA, MARCH-MAY

FIGURE 3.—Reproduced from Brennecke (23).

Stykkisholm. We can also compare Wiese's results based on ice conditions in Greenland Sea region with the findings by Meinardus for ice off Iceland. The spring, summer, and autumn pressure differences in millimeters between Stykkisholm and Vardo for the E+, E−, and all years (normal) are respectively:

	Spring			Summer			Fall		
	E+	E−	Normal [1]	E+	E−	Normal	E+	E−	Normal
Wiese	1.5	−3.5	_____	−1.0	−0.5	_____	−2.0	−2.0	_____
Meinardus	1.3	−2.2	−1.1	−1.1	−1.6	−1.6	−3.0	−2.2	−1.6

[1] The normal for the period 1866–1900.

The agreement of trends is close but its significance is limited by the fact that the ice regime at Iceland proper differs markedly from that in the Greenland Sea region. Incidentally it appears that the pressure difference during the summer and fall, shows the same trend for both types of ice conditions. However the pressure difference itself (normally negative) is accentuated during August to November.

In 1928, Frommeyer (11) investigated the variation in ice conditions in the Spitzbergen region during the period 1898–1921 with the pressure difference between Vardo and Stykkisholm. Frommeyer found agreement in sign of departure and trends in 16 out of 23 cases, while for the 10 years characterized by large variations in ice conditions, the agreement held throughout. The nature of the apparent relationship is: When heavy ice conditions prevail in the Spitzbergen region during May to August the pressure difference Vardo-Stykkisholm tends to be small (normal difference is positive) and, conversely, large, when ice conditions are light. A simultaneous correlation of the pressure difference with ice area gave a coefficient of −0.63. (S. E.=0.21).[4] It was also pointed out that an abnormal pressure difference existed during several years not characterized by exceptional ice conditions.

In 1932, Brooks (25) investigated the bearing of ice conditions in Davis Strait ("storis" ice) on the subsequent distribution of pressure in the neighborhood of the British Isles. The severity of the ice conditions was estimated on a scale 0–10. He found an excess of pressure in western

[4] The following comments by L. F. Page explain the probability indications attached to all correlation coefficients in this report:

"In the papers reviewed, most of the correlation coefficients were followed by a probable error calculated from the formula P. E.$=\frac{0.6745\,(1-r^2)}{\sqrt{n}}$. The distribution of r when ρ, the true correlation in the parent population, is zero is nearly normal, but it has been shown by Fisher (Metron, I, pt. 4, pp. 1–32; 1921) that as ρ increases the distribution becomes more and more skew. Hence an equal range above and below a value of r does not represent equal probabilities in both directions and has no real meaning. Further, in the above formula, n should be replaced by n−1 and r by ρ, which is not known. Fisher (Statistical Methods for Research Workers) suggests the use of z, a function of r, whose distribution depends only on n, but it was thought this would introduce too much confusion to readers not familiar with this concept.

"We are interested here in knowing within certain probability limits, whether the relationships found are real, that is, whether they exist in the parent population of similar data. We can, therefore, assume the real correlation to be zero and estimate the probability of getting by chance, in a sample of the size used, a correlation coefficient of the value found. If the true correlation is zero, the formula becomes P. E.$=\frac{0.6745}{\sqrt{n-1}}$ or S. E.$=\frac{1}{\sqrt{n-1}}$.

The latter has been used, in conformity with modern statistical practice. Tables of probabilities associated with $\frac{z}{\sigma z}$ are available in most texts on statistics and many other places. Some indication may be obtained from the following probabilities: $\frac{z}{\sigma z}=2$, P=0.046; $\frac{z}{\sigma z}=3$, P=0.003; $\frac{z}{\sigma z}=4$, P=0.0001.

"It should be emphasized that in many cases, especially where lags are introduced, correlations are calculated for many combinations of the factors, and the highest are chosen to support a theory. This obviously increases the actual probability of obtaining apparently significant values. A further source of error lies in the fact that in many cases the separate observations of each element are not independent, either because of trends or persistence effects. The formula for the standard error is calculated on the assumption that the items are independent and it is easy to see that if n were reduced to the number of independent observations, the standard error would be increased and the probability of obtaining high correlations by chance would be increased."

Europe during July to December associated with ice in Davis Strait a year and a half before. The figure giving the average change of pressure corresponding with an increase in Davis Strait ice from a scale value of 1 to 10 is reproduced in figure 4. The relationship was indicated from a correlation of the two elements and it will be treated subsequently in more detail, together with other similar relationships involving different time intervals.

In the paper "On the variation of the North Atlantic Circulation and its Consequences" (26) Meinardus treated the variation in the annual pressure difference between three pairs of stations, Copenhagen-Stykkisholm, Ponta Delgada-Stykkisholm, and Toronto-Ivigtut, with ice conditions near Newfoundland. The author computed the pressure difference for the three pairs of stations averaged separately for the five degrees of intensity in ice

AVERAGE CHANGE IN PRESSURE, JULY-DECEMBER, 1 1/2 YEARS FOLLOWING INCREASE IN DAVIS STRAIT ICE FROM INDEX OF 1 TO 10
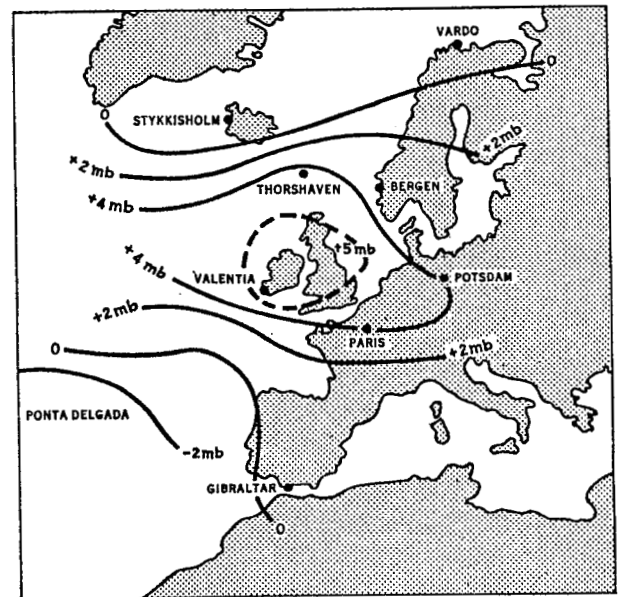
FIGURE 4.—Reproduced from Brooks (25).

conditions. The values obtained as shown below indicate that when ice is in abundance near Newfoundland the pressure difference normally positive is increased, and vice versa.

Character of the ice years off Newfoundland and corresponding pressure departures

Intensity of ice	Toronto-Ivigtut	Number of cases	Ponta Delgada-Stykkisholm	Number of cases	Copenhagen-Stykkisholm	Number of cases
	mm.		mm.		mm.	
2	2.1	3	3.5	4	2.9	6
1	.5	8	.9	10	.6	11
0	.0	7	.3	10	.2	10
−1	−.8	3	−1.2	5	−1.4	7
−2	−1.9	5	−3.5	6	−3.4	7

Kissler (27) treated the relationship between ice conditions in the North Atlantic, 50° W.–70° E., and the spring pressure difference between Copenhagen and

Stykkisholm, over the period 1892–1931. He found for the mean pressure differences for the 9 heavy, E+, and 8 light, E−, ice years respectively: E+, 0.6 millimeter, E−, 4.1 millimeter. The above result indicates a tend-

statement however is limited by the fact that the ice regime over that region is not uniform.

Defant (28) correlated ice at Iceland with the yearly meridional pressure gradient over the North Atlantic as

AVERAGE PRESSURE DISTRIBUTIONS FOR 7 HEAVY AND 4 LIGHT ICE YEARS FOR SPRING, SUMMER AND AUTUMN. ICE SEASON, APRIL TO JULY

LIGHT ICE
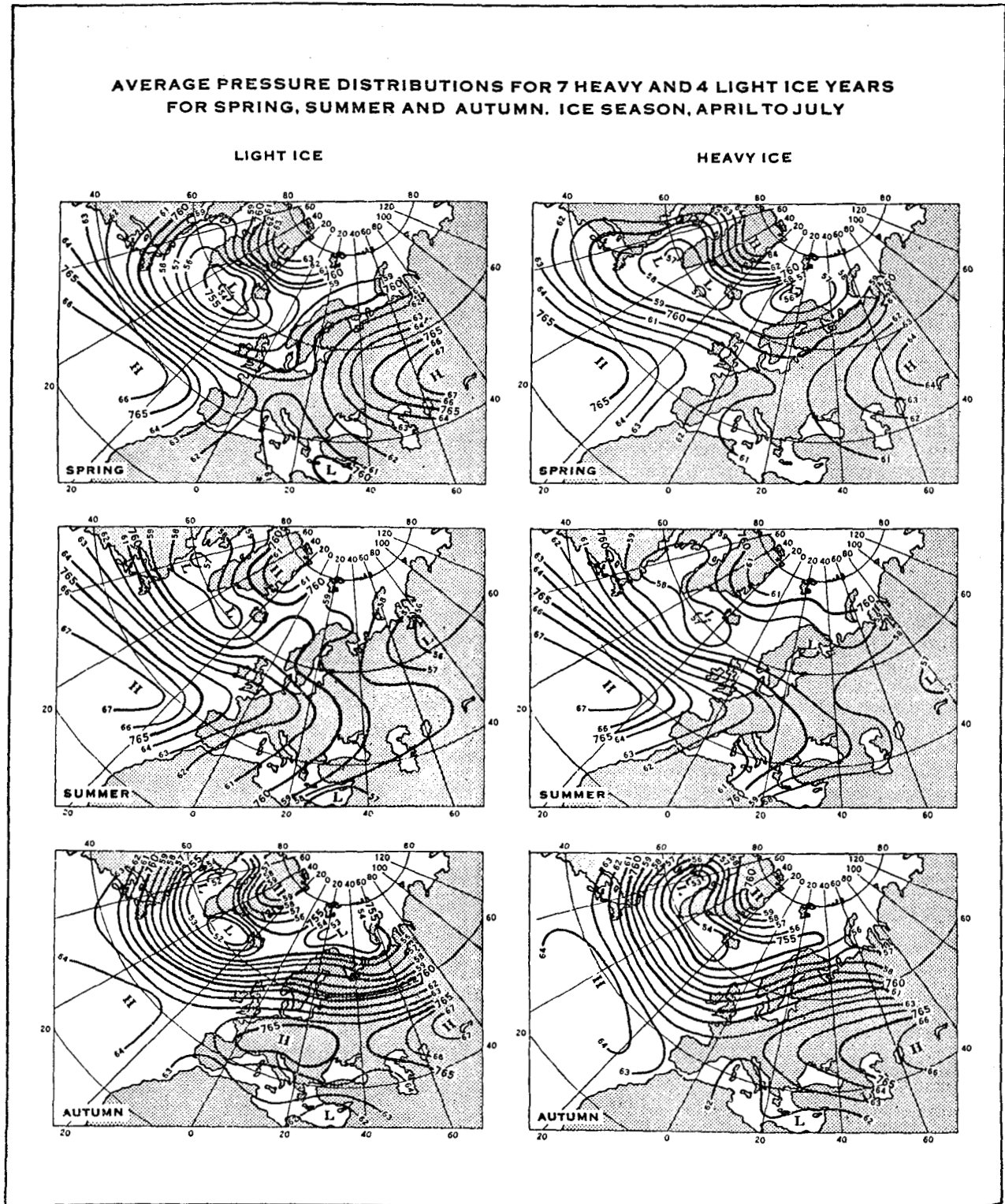
HEAVY ICE

FIGURE 5.—Reproduced from Wiese (2).

ency for pressure to be relatively high at Stykkisholm and low at Copenhagen when heavy ice conditions prevail in the region between 50° W. and 70° E.; and conversely, relatively low and high, respectively, when the ice conditions are light. The significance of the above

well as the west-east gradient over Northern Europe. He obtained respectively r=−0.59 (S. E.=0.21) and r=0.71 (S. E.=0.18).

With regard to the table above and Defant's result I must emphasize the fact that the pressure differences

represent annual values while the average ice season at Newfoundland is from February to May, and at Iceland from January to July, and hence they represent much shorter periods. The disparity in the time intervals of pressure difference and ice seasons limits the significance of the above results.

A considerable advance in the studies of variation in pressure with ice conditions was made when synoptic maps began to be employed. Instead of limiting oneself to isolated points, or to pairs of such, a simultaneous picture of the pressure element over a large area was obtained. The ice region mainly considered is the Greenland Sea.

Brennecke, in the paper previously referred to (23), gave charts of the March–May pressure distribution over
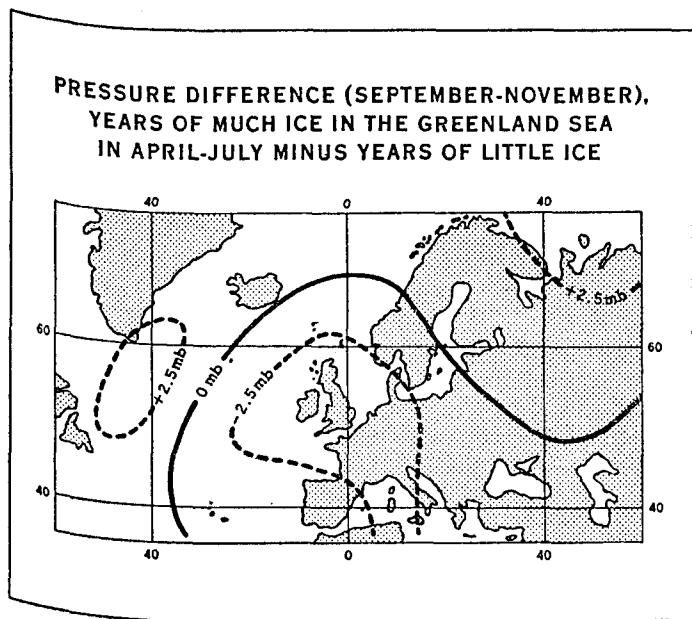
FIGURE 6.—Reproduced from Brooks (4).

the North Atlantic 40°–75° N. and adjoining regions, for each of the 6 abnormal ice years. (See fig. 3.) In 1884 and 1889 (light ice years) the pressure minimum, normally along the west coast of Scandinavia has completely disappeared. Instead, there exists a single depression southwest of Iceland. The pressure gradient between Greenland and northern Norway is practically zero, in contrast to the normally pronounced gradient. In 1881, 1882, 1891, heavy ice years, the Norwegian LOW has either become intensified or suffered a southward displacement. In either case the Greenland to northern Norway pressure gradient is above normal, in 1881 and 1882, apparently due to an intensification of the LOW off the coast; and in 1888 and 1891 mainly because of strengthening and southeasterly shift of the Greenland anticyclone. Another common feature of all the heavy ice years is northerly winds between northern Norway and Greenland as contrasted with easterly winds for the light ice years. In other respects the pressure distributions for the two types of ice years differ little from each other. However there is a tendency for the south-north pressure gradient (Azores-Iceland) to be less in heavy ice years. This is especially evident in 1888 and 1891, when the Icelandic LOW has practically disappeared.

With more extensive material available Wiese (2) averaged the pressure for 7 heavy and 4 light ice years, respectively. Like Brennecke, he employed the ice variation in Greenland Sea region; however, in addition

to the spring pressure distribution, he gave charts for the summer and autumn. (The ice season ends in July.) The area considered is also more extensive. It includes a larger expanse of the North Atlantic and all of Europe eastward to 70° E. longitude.

There appears to be a close correspondence between Wiese's results and those obtained by Brennecke for the spring season. (See fig. 5.) With the E— group, the
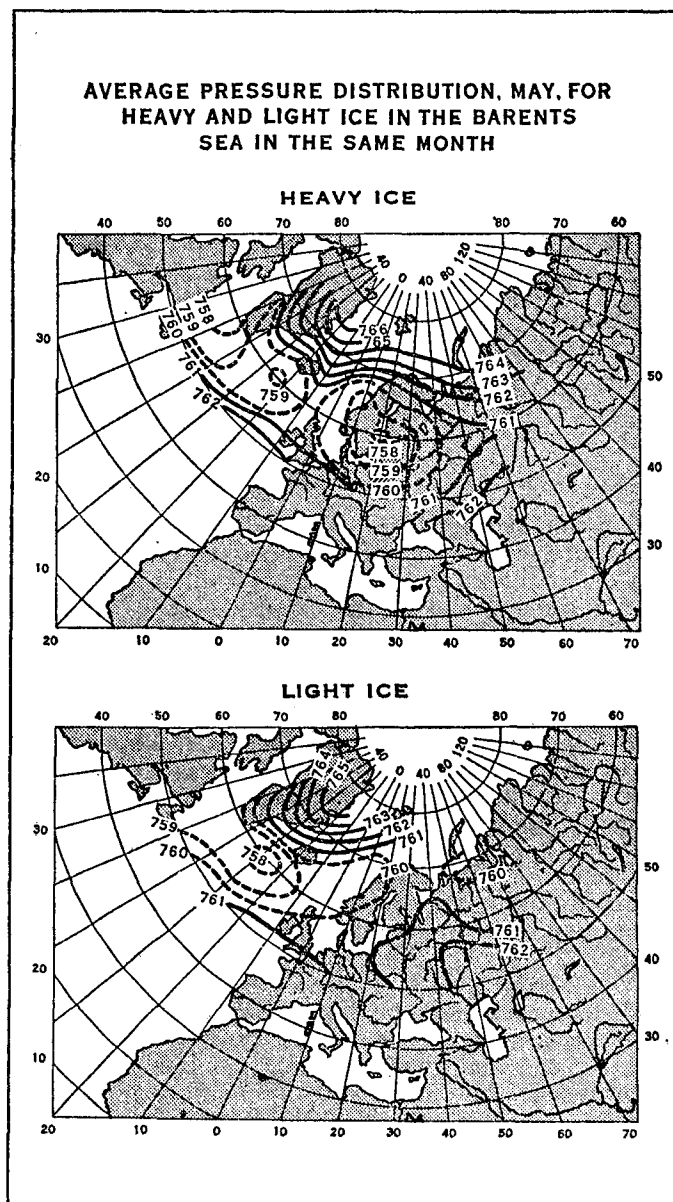
FIGURE 7.—Reproduced from Wiese (6).

Norwegian LOW is absent; for the E+ it is very intense, in fact, deeper than the Icelandic LOW. The relationship between the pressure difference between upper Scandinavia and Greenland, found by Brennecke, is substantiated. The tendency for a small south-north pressure gradient for the E+ group is here very definite.

Comparing further the E+ and E— maps, one finds for the first group higher pressure in the vicinity of Iceland and Greenland and lower pressure elsewhere, with the exception of the British Isles. A general feature of the E+ maps is a filling up of the low pressure centers in the North Atlantic and a flattening of the Azores HIGH and the high pressure field over eastern Europe and also of the

TABLE 4.—*Relationships between ice and approximately contem-*

[Summary of the main

	Region	Ice season	Author	Period of years used	Number of ice years		Pressure in ice years	
					Heavy (E+)	Light (E—)	Place or region	Months affected
A	Greenland Sea and Iceland.	January-July	Brennecke	1877-95	4	2		
A	Greenland Sea	April-July	Wiese	1880-1916	7	4	Greenland Sea, Iceland regions / British Isles / Northern Norway, Norwegian Sea and elsewhere.	March-May / do / do
A	...do	...do	...do	1880-1916	7	4	Barents Sea / British Isles	June-August / do
B	...do	...do	...do	1880-1916	7	4	Iceland, Greenland, Barents and Kara Seas / Western Europe	September-November / do
A	Iceland	January-July	Meinardus	1866-1900	7	6		
B	...do	...do	...do	1866-1900	7	6		
A	...do	...do	Brooks	1901-19	43 months 701 days		Stykkisholm	January-July (probably)
A	...do	...do	Defant	1880-1905				
A	Barents Sea	April-August (only May used).	Wiese	1896-1916	3	5	Greenland and Barents and Kara Seas / Baltic region	March-May / do
A	50° W.-70° E	April-August	Kissler	1898-1931	9	8		
A	Spitzbergen	...do	Frommeyer	1898-1921	7	3		
B	Davis Strait	January-July	Brooks	1895-1927			Western Europe	July-December following year.
A-B	Newfoundland	February-May	Meinardus	1860-1902	14 / 17 / 11	11 / 14 / 8		

Siberian maximum. This brings about a diminution of the pressure gradient over Europe and the North Atlantic south of latitude 60°. The pressure difference between the Icelandic LOW and the Azores HIGH and 60° E., 50° N., respectively, is about 8 millimeters and 9 millimeters for the E+ years as compared with 12 millimeters and 13 millimeters for the E— group. On the other hand a considerable pressure gradient is maintained north of Iceland, because of the rise in pressure over Greenland and its vicinity and a deepening over the Norwegian Sea. Thus between northern Scandinavia and Greenland the difference is the larger for the E+ group. (See table 2.)

Less marked differences appear from a comparison of the summer maps. The mean pressure is lower over Great Britain and Scandinavia and markedly higher over Barents Sea for the E+ group. The pressure gradients do not on the whole differ in the two cases.

During the autumn (well past the ice season) the pressure distribution over the North Atlantic and Europe is characterized in the E+ group by a comparatively higher pressure in the vicinity of Iceland, Greenland Sea, Barents Sea but considerably lower pressure over western Europe. (See figs. 5 and 6, prepared by Brooks from Wiese's results.)

Wiese (6) also constructed maps giving the May pressure distribution for years with heavy and light ice during that month in Barents Sea. Comparing the two pressure distributions (see fig. 7) he found for the E+ group higher pressure over Greenland region but notably over Barents and Kara Seas, and much lower pressure in the Baltic region. This appears to be due to an extension of the Greenland anticyclone far eastward, while the seat of lowest pressure is shifted from Iceland in the same direction. The north-south pressure gradient over Barents Sea becomes steep.

Brennecke and Wiese described at length the variation in position of centers of low and high pressure and the configuration of the pressure fields with ice conditions. Wiese's results, based on averages of several years, differ little in essentials from Brennecke's. They can be described as follows. (See fig. 5.)

In spring, the Icelandic LOW for the E+ group is in the same position as for the E— group, but is supplemented in the first instance by a low in Davis Strait and another off the Norwegian coast. Comparatively speaking, the Azores HIGH appears to have retreated southward (see position of 765 millimeters isobar) but its northeastward extension (761 millimeters isobar) is farther north. The Siberian HIGH has retreated eastward. Associated with this difference in position of pressure centers is a difference in orientation of isobars.

In summer, a comparison of the E+ group with the E— group shows an extension of the Polar high-pressure field over Barents Sea, a displacement southward of the west Siberian LOW which extends into European Russia, a retreat of the northeastward extension of the Azores HIGH, and a new area of low pressure over the Scandinavian Peninsula.

During the autumn we have for the E+ years a southeastward displacement of the Greenland high-pressure center and an extension, in the same direction, of the Icelandic trough of low pressure. The feature of the E— map is the presence of a high-pressure area over Europe and a corresponding shift of isobars northward. A notable difference in the configuration of the isobars is shown over the mid-Atlantic area. In the latter instance they run in a southwesterly direction as contrasted with a westerly orientation in the E+ group.

Added information bearing on the above results as well as indicating other relationships is contained in the attempt made by Brooks and Quennell (4) to determine statistically the possible influence of the Arctic ice on the subsequent pressure distribution over the Eastern North Atlantic and western Europe. All available ice data extending over periods varying in general from about 30 to 40 years from several regions in the North Atlantic and Arctic waters were correlated separately and jointly with pressure and other elements at nine stations distributed over western Europe, Iceland, and Greenland. They are: Jacobshavn (West Greenland), Stykkisholm, Thorshavn (Faroes), Vardo, Bergen, Valentia (Ireland), Paris, Berlin, and Ponta Delgada (Azores).

porary (A) and subsequent (B) pressure and pressure difference
results presented above]

Pressure in ice years				Pressure difference in ice years			
Element of pressure	Heavy ice season	Light Ice season	Place of region	Months affected	Element of pressure difference	Heavy ice season	Light ice season
			Greenland minus Northern Norway	March–May	Mean	Larger	Smaller.
Mean	Higher	Lower	Azores minus Iceland	do	do	Smaller	Larger.
...do	...do	...do	Greenland minus Northern Norway	do	do	Larger	Smaller.
...do	Lower	Higher	Azores minus Iceland	do	do	Smaller	Larger.
			North of Iceland minus Iceland	do	do	do	Do.
...do	Higher	Lower	Europe, North Atlantic	June–August	do		Little difference.
...do	Lower	Higher					
...do	Higher	Lower	South of subpolar North Atlantic Low	September–November	do	Smaller	Larger.
...do	Lower	Higher	Stykkisholm minus Vardo	March–June	do	Smaller (normal negative difference diminished).	Do.
			do	August–November	do	Greater than normal with both E+ and E—.	
Mean	Higher than normal						
			North Atlantic south-north	Year	do	Smaller	Larger.
Mean	Higher	Lower	Northern Europe, west-east		do	Larger	Smaller.
...do	Lower	Higher	Barents, Kara Seas (north minus south)	March–May	do	do	Do.
			Stykkisholm minus Vardo	do	do	Smaller (normal negative difference diminished).	Larger.
Mean			do	May–August	do	do	Do.
	Excess of pressure in E+ as compared with E—.						
			Ponta Delgada minus Stykkisholm	October–September	do	Larger	Smaller.
			Copenhagen minus Stykkisholm	do	do	do	Do.
			Toronto minus Ivigtut	do	do	do	Do.

Quarterly means of pressure were employed. A large number of correlation coefficients involving relationships with relatively few variates but with varying time lags were computed. In a few cases where the record was fairly long, more than 50 years, the observations were not homogeneous, but in such cases two homogeneous periods can be distinguished. The authors therefore thought best to carry out separate correlations for the two series of observations and consider their weighted mean. Often a disagreement between the two sets of coefficients was found. The number of significant coefficients, those whose value is greater than might arise by chance, is very small. However, it should be noted that a physical basis appears to exist for many of the relationships, so that even some of the small coefficients were regarded by the authors as significant provided the record was long.

On the whole the relationships which were either suspected from or indicated by the pressure distributions were borne out by the correlations. A number of relatively large coefficients suggested relationships involving long-time intervals, up to four and a half years.

In closing their investigation Brooks and Quennell draw the following conclusion:

The general results of this investigation of the effect of ice conditions on subsequent pressure in the eastern North Atlantic and western Europe is that Arctic ice is an appreciable factor in the weather of the British Isles. Much ice in the spring and summer tends to be associated with high pressure in the same months at the northwestern stations Jacobshavn, Stykkisholm, and Thorshavn, and with low pressure at the southern stations Paris and Ponta Delgada, the relationships being shown by well-supported correlation coefficients which range up to 0.5. Again, much ice in spring or summer tends to be followed in November to January by low pressure over the British Isles, this relation being very definite and regular whatever index of Arctic ice conditions is employed. The ice conditions in various parts of the Arctic have been combined into a series of "ice index" figures, which have the following well-supported correlation coefficients with pressure in the following November to January: Ponta Delgada —0.35, Stykkisholm +0.27, Ponta Delgada minus Stykkisholm —0.37. These relationships appear to recur in the following 2 years, thus giving rise to a rather regular annual variation of the correlation coefficients * * *. The effect of the ice off Newfoundland on pressure over western Europe is generally similar to that of Arctic ice, but is much less pronounced.

In a subsequent discussion (29) of the above investigation, C. E. P. Brooks added:

The influence (of ice on the pressure distribution) varying with the season in a way which suggests that it is due to a combination of several factors, some acting in one direction, some in another. As a result the correlation coefficients obtained while sometimes appreciable are never high, though they are sufficiently confirmed by various checks to show that they are real.

Brooks (25) also correlated ice with overlapping three-monthly means of pressure at nine stations—

beginning with July to September of the ice year and continuing to the end of the second year.
The first point to notice is that towards the end of the year to which the ice measurements refer, there is a negative correlation between ice and pressure at Stykkisholm in Iceland, and positive correlation between ice and pressure at Bergen, Paris, Berlin, and to a less extent, Gibraltar.
In the second half of the following year there is a prolonged hump over western and central Europe, extending from Bergen in the north to Gibraltar in the south, and from Valentia in the west at least as far as Berlin in the east; at Paris, in the centre of this region, the correlation coefficient rises to +0.57 in August to October.

Temperature.—Generally, along with the studies of relationships between ice and pressure, a connection between the former and air and surface-water temperatures was also sought. It was found that, in general, low temperatures were associated with heavy ice conditions and, conversely, high temperatures with light ice conditions. It also appeared that the biggest negative departures occurred before the height of the ice season and that the low or high temperatures were characterised by a certain persistence; furthermore, the association of ice with temperatures was evinced in regions beyond the ice boundary.

Brennecke (23) computed the spring air temperature departure at Stykkisholm and the water temperature departure at Papey (east Iceland) for each year of the period 1877–95. (See fig. 2.) The two curves which, on the whole, show good agreement with each other and with the pressure difference curve, definitely indicate lower temperatures during the spring at the two stations for heavy ice seasons and, conversely, above normal temperatures for light seasons. With the exception of the

year 1889, this fact is especially apparent with years characterized by exceptionally abnormal ice conditions. The negative departure of the water temperature in 1889, an abnormally light ice year, is due, according to Brennecke, to the preceding series of heavy ice years (1886–88).

Brennecke further found lower air temperatures at Bodö on the Norwegian coast, during the spring and summer of heavy ice years and the same trend in the water temperatures at Thorshavn and Ona during the 2 very heavy ice years 1881 and 1888. Conversely, higher air temperatures were observed during a light ice season.

Meinardus (10) extended the above investigation relative to ice conditions at Iceland and computed the differences between the mean monthly temperatures during heavy and light ice years as well as the departures from the mean at Grimsö, Stykkisholm, Papey, Thorshavn, Copenhagen, Greenwich, and some stations on the Norwegian coast. Grimsö, an island of the north Iceland coast, shows clearly a large difference in air temperature between the opposite types of years. (See table 5.) With E+ years the departure from the mean monthly temperature gradually increases, beginning in October and, after attaining a maximum value in March, maintains a departure from normal of same sign through the summer. The same is true to a large extent of the E− group. The largest monthly contrast in departures between E+ and E− years occurs in March, 7.3°.

Stykkisholm (west Iceland) values show similar trends but smaller departures, while Thorshavn (Faroes), to the east, also retains the above characteristics to a certain degree. Neither Greenwich nor Copenhagen values show any marked difference between the E+ and E− groups.

The negative water temperature departure at Papey during heavy ice years gradually increases to a maximum in June (−2.6°). Though the positive departures are generally smaller the difference between them, Meinardus points out, attains a value above 3°. Thorshavn water temperature shows the same trend, and like its air temperature, a smaller variation than Papey. The stations on the Norwegian coast, with the exception of the period from March to June, fail to show a definite tendency in the temperature regime and even for that season it is very slight.

Wiese (30) averaged the winter temperatures at Leningrad, separately, for the 27 heavy and the 29 light ice years reported at Iceland during the period 1752–53 to 1881–82 (1801–4 missing). He found:

Mean Leningrad temperature (degrees centigrade)

	XI	XII	I	II
After a heavy ice season at Iceland	−2.0	−8.0	−11.8	−8.5
After a light ice season at Iceland	−1.2	−5.4	−8.8	−8.3
Difference	−.8	−2.8	−3.0	−.2

TABLE 5.—*Temperature (degrees centigrade) departures during heavy ice (E+) and light ice (E−) years*

AIR TEMPERATURE AT GRIMSO (1874-1902)

Type	Number of years	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
E+	5	−0.1	−0.8	−1.8	−3.3	−4.0	−4.6	−1.8	−1.7	−1.8	−2.0	−2.3	−0.5	0.6	0.7	0.3
E−	6	.1	.8	1.0	.8	1.7	2.7	1.8	1.3	.9	1.0	1.6	.6	.0	.3	.3
Difference		−.2	−1.6	−2.8	−4.1	−5.7	−7.3	−3.6	−3.0	−2.7	−3.0	−3.9	−1.1	.6	.4	.0

AIR TEMPERATURE AT STYKKISHOLM (1850-1902)

Type	Number of years	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
E+	9	−0.6	−0.2	−0.4	−2.1	−3.0	−4.4	−2.0	−1.5	−0.7	−1.0	−1.3	−0.8	0.4	−0.6	−0.9
E−	10	−.1	.4	.2	1.0	1.2	1.8	1.9	1.0	.2	.3	.5	.7	.2	.1	1.2
Difference		−.5	−.6	−.6	−3.1	−4.2	−6.2	−3.9	−2.5	−.9	−1.3	−1.8	−1.5	.2	−.7	−2.1

AIR TEMPERATURE AT THORSHAVN (1867-1902)

Type	Number of years	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
E+	7	−0.4	−0.6	−0.3	−0.3	−0.4	−0.7	−0.6	−0.7	−0.8	−0.5	−0.8	−0.4	−0.1	0.3	−0.3
E−	6	.3	.5	.6	.9	−.1	1.1	.7	1.1	.3	−.2	.7	.4	.0	.2	.6
Difference		−.7	−1.1	−.9	−1.2	−.3	−1.8	−1.3	−1.8	−1.1	−.3	−1.5	−.8	−.1	.1	−.9

WATER TEMPERATURE AT PAPEY (1875-1902)

Type	Number of years	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
E+	5	−0.3	−0.2	−0.8	−0.9	−1.3	−1.3	−1.9	−2.5	−2.6	−2.1	−1.6	−1.3	−1.0	−0.3	0.1
E−	5	−.2	.2	.6	.7	.5	1.3	1.4	1.0	1.0	.9	1.1	1.0	.9	.6	.8
Difference		−.1	−.4	−1.4	−1.6	−1.8	−2.6	−3.3	−3.5	−3.6	−3.0	−2.7	−2.3	−1.9	−.9	−.7

WATER TEMPERATURE AT THORSHAVN (1876-1902)

Type	Number of years	X	XI	XII	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
E+	5	0.0	−0.1	0.4	−0.2	−0.5	−0.3	−0.6	−0.8	−0.6	−0.4	−0.5	−0.2	−0.3	−0.1	0.2
E−	5	−.1	.0	.4	.4	.1	.3	.4	.5	.2	.4	.4	.6	.2	.2	.1
Difference		.1	−.1	−.8	−.6	−.6	−1.0	−1.3	−.8	−.8	−.9	−.8	−.5	−.3		.1

Wiese (31) correlated the area of ice in the Greenland Sea with the mean monthly water temperature at Papey. The following values of r were obtained for the period 1882–1915. (S. E.=0.17)

	March	April	May	June	July	August	September
r=	−0.17	−0.30	−0.36	−0.45	−0.41	−0.37	−0.37

The highest values are for June and July. This agrees with results obtained by Meinardus who used Iceland ice data. In a previous paper Wiese (2) discusses the spring temperature regime at Markree Castle, Dublin, Valentia, Douglas, and York in the British Isles. He found no variation in the mean temperature at the above stations with ice conditions in Greenland Sea. This, says the author, is probably due to the fact that the colder continental air masses arriving during a heavy ice season undergo much warming during the day time. However, taking

the mean absolute departures of the temperature from normal he obtained for the heavy ice years, E+, ±0.79° C.; for the light ice years ±0.17° C. Similarly for the interdiurnal variability of temperature (9 a. m.) at Douglas alone, based on a 35-year average, Wiese obtained:

	E+	E—
	°C.	°C.
April	±1.47	±1.17
May	±1.93	±1.41

The dependence of the temperature at Douglas on the state of ice in the Greenland Sea is very definite and can be traced also for single years. A correlation between the ice area in the Greenland Sea and interdiurnal variability
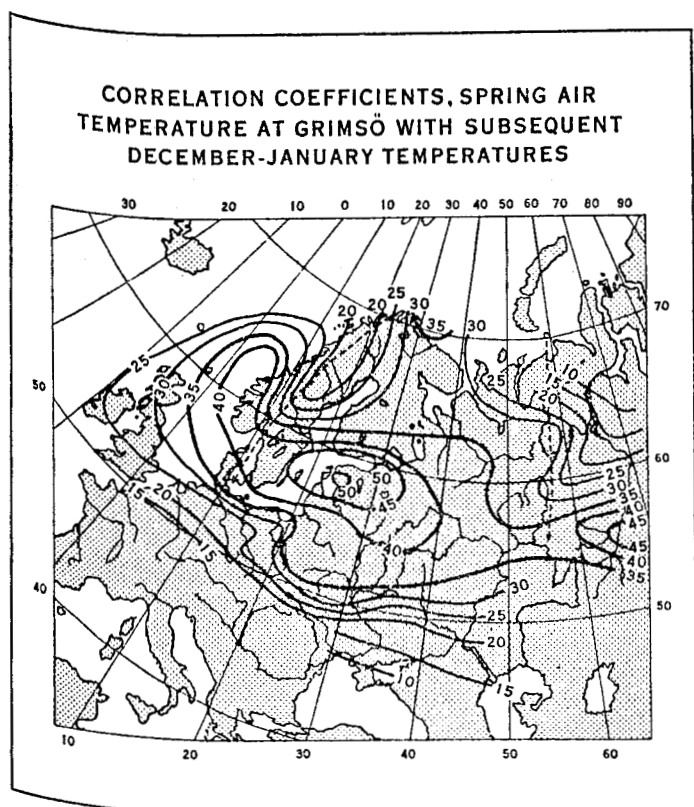

CORRELATION COEFFICIENTS, SPRING AIR TEMPERATURE AT GRIMSÖ WITH SUBSEQUENT DECEMBER-JANUARY TEMPERATURES

FIGURE 8.—Reproduced from Wiese (31).

in temperature at Douglas for April to July gave r=0.73 (S. E.=0.19). Wiese also found that the average last day of frost for that station is later for E+ than for E— by 11 days, and that during the 32 years investigated frost never occurred in May during the light ice years but was observed when ice conditions were heavy. Also for the mean October and autumn temperature in Scotland, Wiese found the relation shown by the figures below:

	E+	E—
	°C.	°C.
October	−0.6	0.8
Autumn	−.2	.4

Wiese (31) next sought a connection between December to January temperatures in Europe and ice conditions in the Greenland Sea during the preceding season. However, instead of taking the incomplete ice figures he chose

the mean spring air temperature at Grimsö which appears to be (see table 5) intimately related to the ice regime. A correlation of the Grimsö temperature with the subsequent December to January temperatures at 59 stations in Europe, in most cases over the period 1882–83 to 1917–18 showed (see fig. 8) a large uniform area of correlations which forms a regular system. The standard error of these correlations is 0.17.

Walker (32) using a slightly different period (including 1880, 1881, but omitting 1896, the latter for lack of data) correlated the spring temperature at Grimsö with the subsequent winter temperature December to February, at Christiansund, Vardö, and Berlin and obtained for r the values 0.0, 0.08, and 0.10. When he used the years employed by Wiese he obtained for the winter months 0.25, 0.44, and 0.31 respectively. The above coefficients, by themselves, admit no definite conclusions. Their standard errors are 0.17.

CORRELATION, BARENTS SEA ICE, MAY-JUNE, WITH CONTEMPORARY TEMPERATURE
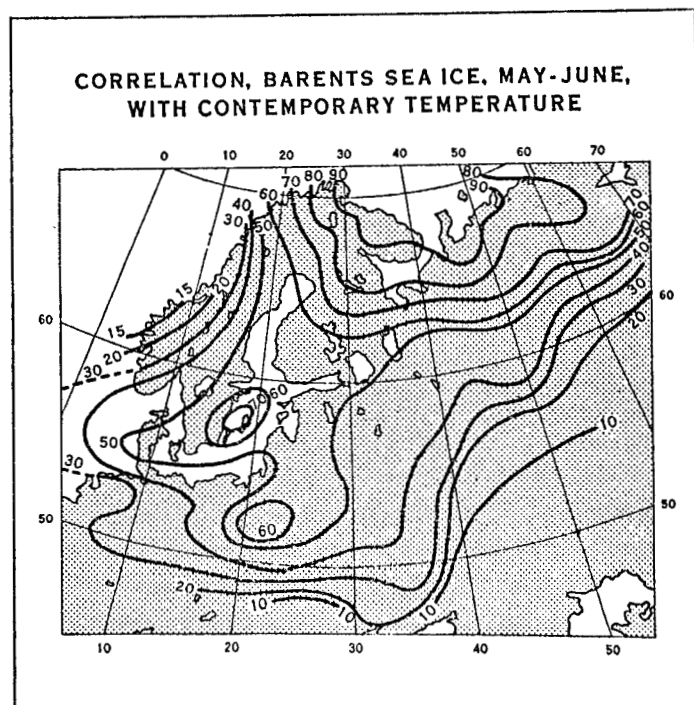
FIGURE 9.—Reproduced from Wiese (34).

Wiese (33) correlated the ice area in Barents Sea during July to September with the subsequent winter water surface temperature in that region. He obtained a coefficient of r=−0.79 (S. E.=0.20).

A study (34) of ice conditions in Barents Sea during May to June for a period of 21 years and contemporary mean temperature in Europe at 69 stations, shows high correlations for the Murman coast (Teriberka, r=−0.82, Vardö, r=−0.81) and also relatively high values at other points in Europe, especially for the east coast of Sweden and Poland. Thus, for Stockholm, r=−0.49; Visby, r=−0.60; Kalmar, r=−0.46; Lund, r=−0.48; Pinsk, r=−0.47; Warsaw, r=−0.58; Lwow, r=−0.50, all with standard errors of 0.22. (See also map of isocorrelations, fig. 9.) The reason for the concentration of high correlation values along a narrow zone is, according to Wiese, due to the fact that it is immediately west of the path of the cyclones which, during years with heavy ice, move northward from the Mediterranean, between longitudes 30°–40° E. and hence is directly related to ice conditions in Barents Sea.

	Region	Ice season	Author	Period of years used	Number of ice years		Air temperature in ice years	
					Heavy (E+)	Light (E−)	Place or region	Months affected
A	Greenland Sea and Iceland.	January–July	Brennecke	1877–95	4	2	Stykkisholm	March–May
A	Greenland Sea	April–July	Wiese	1880–1918	7	4	Bodo	March–August
B	do	do	do	1880–1916	7	4	British Isles	March–May
B	Grimso	Spring temperature (indicative of ice).	do	1882–1917			Douglas	Autumn
							Europe, 59 stations	December–January
B	do	do	do	1882–1917			Europe, 3 stations	December–February
B	do	do	Walker	1880–1917			Europe, same stations	do
A	Iceland	January–July	Meinardus	1860–1900	7	6	Grimso	January–September
							Stykkisholm	do
							Thorshavn	January–June
B	do	do	do	1860–1900	7	6		
B	do	do	Wiese	1752–1882	27	29	Leningrad	November–February
A	Spitzbergen	May–August	Frommeyer	1898–1921	7	3	Greenharbor	May–August
A	Barents Sea	May–June	Wiese	1896–1916			Europe, 69 stations	May–June
B	do	July–August	do	1896–1916				

Frommeyer (11) compared three Greenharbour temperature curves (May to June, July to August, May to August) with the corresponding curves of ice distribution in Spitzbergen waters, over the period 1912–21. In almost every year and for every season much ice in that region is associated with low temperatures and, conversely, high temperatures with little ice. A similar treatment of the September and October temperature gave indifferent results.

Cyclonic activity.—Various investigators brought out the fact that the pressure gradients over the North Atlantic, Europe, and elsewhere are generally weakened during years with heavy ice as compared with years having light ice conditions; further, that there is a variation in the position and orientation of the usual pressure fields, the main tendency of which is a southeastward displacement in heavy ice years. It occurred to Wiese to find out (2) whether the cyclonic activity, as given in some cases by the number of cyclones and the mean latitudinal position of the cyclonic path and in others by their actual distribution, varies with ice conditions.

Wiese tabulated separately the average number of spring, summer, and autumn cyclones in the region between 60° W. and 80° E., for the 7 heavy and 4 light ice years (Greenland Sea). Unfortunately, because of an error, the author was unable to draw the logical conclusions from his results. Adding up the figures for the three seasons (6) I found, for the E+ group, 723 cyclones as compared with 470 for the E− group; or reduced to the same unit, 4 years, 413 to 470. Thus the number of cyclones during March–November appears to be less in years characterized by heavy ice conditions.

A comparison of seasonal values also brought out the fact that the difference in number between the two groups increases during the year. Thus we find only a slight difference in the spring. In the summer the number for the E+ group is 90 percent of that for the E− group, in the autumn but 76 percent. (See fig. 10.) Note also in the table below that for the E+ group the number of cyclones in the autumn is considerably smaller than in the spring whereas the E− group shows a slight increase in autumn.

Wiese calculated the average latitudinal paths [5] of cyclones over the same region and for the same years.

There appears to be (see fig. 11) a marked displacement of the mean cyclonic path southward in the summers of heavy ice years and a still more notable displacement in the autumn. For the North Atlantic region (40° W.–50° E.) the average displacement is 2.9° of latitude and 3.4°, respectively. In the autumn the differences is large everywhere east of, and beginning with, 10° W. West of this longitude the difference is less than in the summer. On the other hand, a greater displacement of the mean path southward is shown for the eastern portion in the autumn.

	Number of cyclones			
	E+		E−	E+/E−
	7 years	Reduced to 4 years	4 years	
				Percent
Spring	283	162	165	98
Summer	214	122	136	90
Autumn	226	129	169	76

Wiese also investigated the relationship between ice conditions in Greenland Sea, or rather the spring temperature at Grimsö, and the latitudinal distribution of cyclones in the following December to January, over an area bounded by 40° and 75° N. latitudes, and including in addition to Europe a portion of Western Siberia. He found a greater number of cyclones between latitudes 40° and 60° and a smaller number north of 65° after a cold spring in Grimsö. This probably indicates that also in December to January the mean cyclonic path is displaced southward after a heavy ice season in Greenland Sea, or rather after a cold spring in Grimsö.

Number of cyclones in each zone of 5° latitude December to January after cold and warm spring at Grimsö (average of 5 years)

	40°–45°	45°–50°	50°–55°	55°–60°	60°–65°	65°–70°	70°–75°
After a cold spring	81	36	77	105	100	74	68
After a warm spring	73	21	47	92	100	82	96
Difference	8	15	30	13	0	−8	−28

In another paper (34) Wiese pointed out that the distribution of cyclones over eastern Europe for the month of May appears to vary with ice conditions during the same month in Barents Sea.

[5] The position of the mean path was obtained by determining the intersecting point of each cyclone with every 10° meridian, and computing the mean latitude of the intersection points. Thus the values represent the mean position of the cyclones on the respective meridians. Only those cyclones were considered which either appeared in the North Atlantic or could be traced from America. Lows appearing in the Baltic, Mediterranean, White Sea, Eurasia, as well as stationary lows which had a weak displacement without any definite direction (stationary type) were not considered.

temporary (A) and subsequent (B) air and water temperatures
results presented above]

| Air temperatures in ice years—Continued			Water temperature in ice years				
Element of temperature	Heavy ice season	Light ice season	Place or region	Months affected	Element of temperature	Heavy ice season	Light ice season
Mean	Lower	Higher	Papey	March–May	Mean	Lower	Higher.
----do	----do	----do	----do	March–September	----do	----do	Do.
Interdiurnal varability	Larger	Smaller					
Mean	Lower	Higher					
----do	----do	----do					
----do							
----do	----do	----do					
----do	No relationship evident			January–July	----do	----do	Do.
----do	Lower	Higher	----do	----do	----do	----do	Do.
----do	----do	----do	Thorshavn				
----do	----do	----do	Papey	August–December	----do	----do	Do.
			Thorshavn	August–November	----do	----do	Do.
Mean	Lower	Higher					
----do	----do	----do					
----do	----do	----do	Barents Sea	December–February	----do	----do	Do.
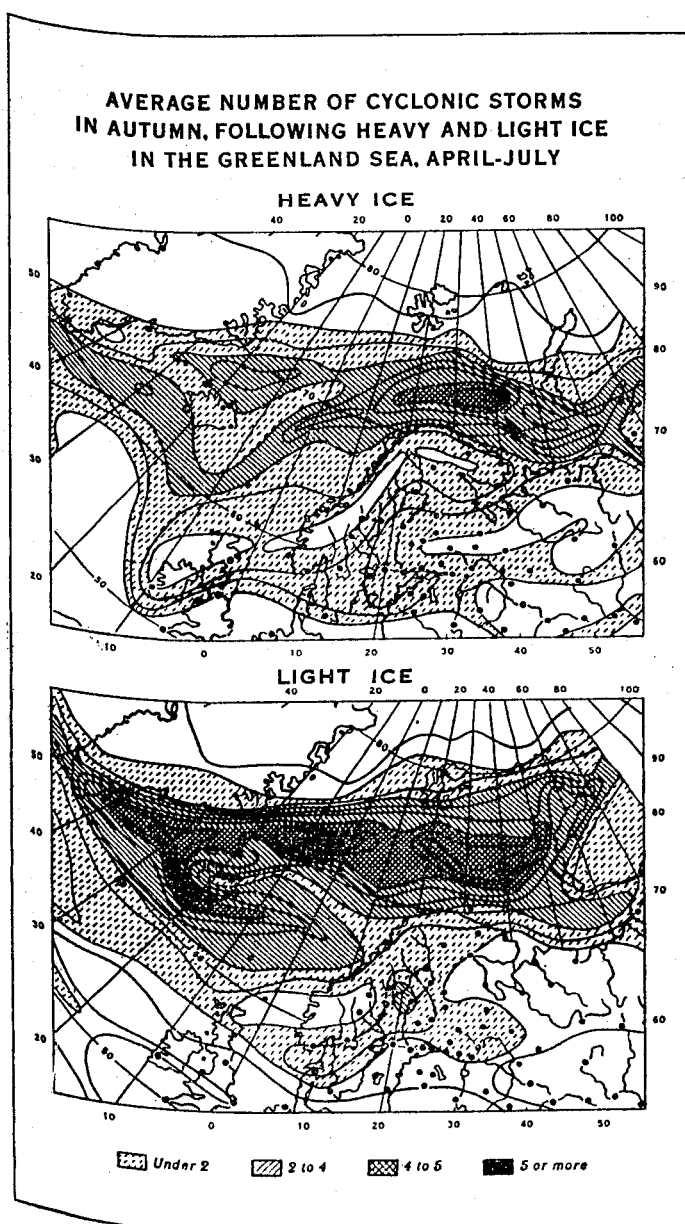
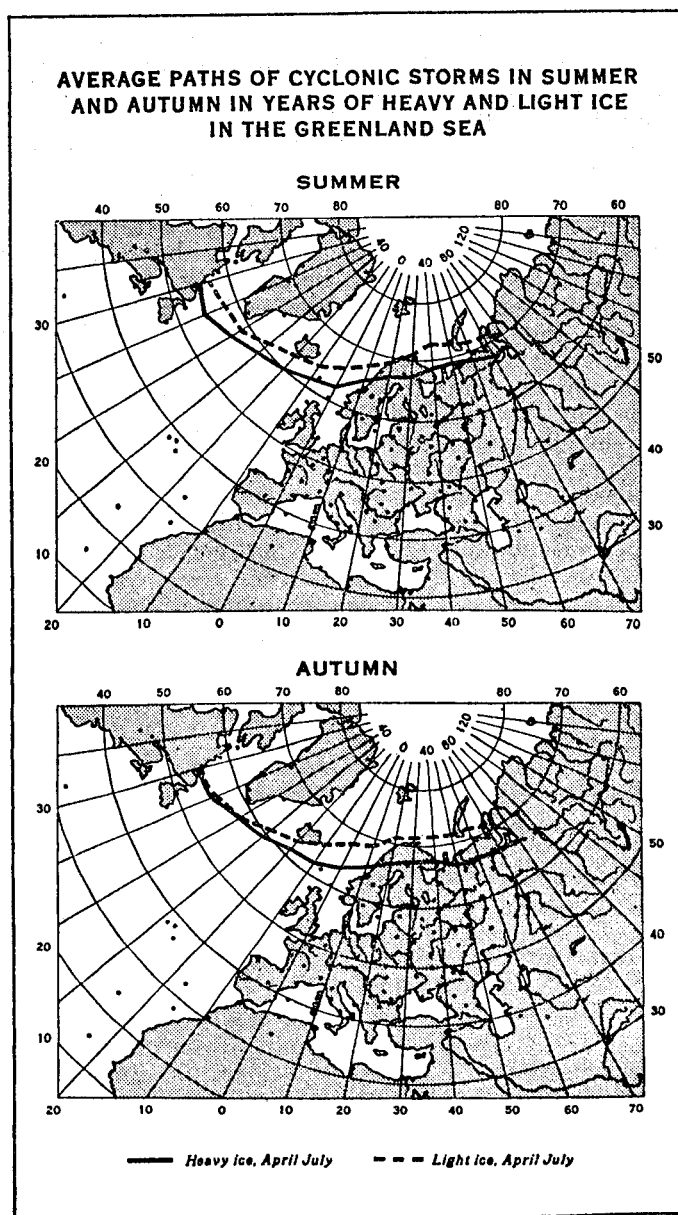FIGURE 10.—Reproduced from Wiese (6).

FIGURE 11.—Reproduced from Wiese (6).

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Summing up Wiese's results it appears that the number of cyclones in heavy ice years is less in the summer and especially in the autumn, than in the respective seasons in light ice years. Similarly, in heavy ice years in the Greenland Sea the cyclones traveling across the North Atlantic Ocean take a more southerly course in the summer, autumn, and early winter than in light ice years. This trend manifests itself somewhat more readily in the western portion of the North Atlantic in the summer and more readily in the eastern portion of that ocean in the autumn.

In years of heavy ice in the Barents Sea there is a greater concentration of cyclones moving from the Black Sea north-northeastward between 25°–45° E. in the month of May than in years of light ice.

Precipitation.—The results presented in the preceding section indicate the possibility of a relationship between ice and precipitation. Wiese (2) tabulated the departures in rainfall for several regions in northwestern Europe which he thought might be affected by ice distribution in the Greenland Sea. (See fig. 10.) The years were divided according to the ice: 8 heavy, E+; 6 moderately heavy, E(+); 6 normal, En; 6 moderately light, E(−); and 5 light, E−, ice conditions. The author pointed out the variation of rainfall with ice; but the analysis of the values promised in a forthcoming paper does not seem to have been published. It would appear, however, from his investigation of the relationship between cyclonic activity and ice that the departures and trends in rainfall as shown by the values below can perhaps be explained in the following manner.

The first three regions in table 7—the Baltic Sea coast, Scotland, and southeast England—represent the zone where the variation in cyclonic paths would be expected to affect the autumn precipitation most. The other two, Ireland and England, and Norway, were chosen to demonstrate the effect on the spring precipitation of changes in ice. Further, the first three regions represent two zones of latitude; the Baltic Sea coast and Scotland stations are located between 56°—59° N., while the stations in Southeast England are, roughly, between 51°—52½° N.

Table 7.—*Departures in rainfall expressed in percentages*

Region	Number of stations	Season	E+	E(+)	En	E (−)	E−	Normal precipitation
Baltic Sea coast (lat. 56°–59° N., long. 13°–27° E.).	8	August	8	3	3	−3	−13	*mm.* 75
Scotland (lat. 56°–59° N., long. 2°–5° W.).	4	Autumn	7	6	−5	−1	−13	248
Southeastern England (lat. 51°–52° N., long. 1° E.–2° W.).	3	October–November.	24	13	−8	−9	−27	161
Ireland and England (lat. 52°–54° N., long. 1°–10° W.).	5	April	−17	−8	−3	5	20	65
Norway (lat. 61°–67° N., long. 5°–14° E.).	5	Spring	10	3	6	−10	−15	210

Considering only the exceptionally abnormal ice years, the first and last columns of the table, one finds for the eight stations along the Baltic and four stations in Scotland more or less the same values of the departures. The same trend as for the above is shown by the three stations in southeast England; however the departures are much greater. The general increase of autumn precipitation in heavy ice years and decrease in light ice years is probably to be expected from the more southerly position of the mean cyclonic path in heavy as compared with light

ice years. Though it is possible that a shift in the mean cyclonic path may not involve an increase in the number of cyclones immediately south of the previous path and a diminution north of it, in our case this seems to be so.

The larger departures for southeast England might be explained by considering the detailed distribution of cyclonic frequency in the neighborhood of the British Isles in December to January of years characterised by exceptionally abnormal ice conditions in the Greenland Sea (preceding April to July). Wiese showed that during these 2 months a greater number of cyclones south of latitude 60° occurs after a cold spring at Grimsö than after a warm spring and that the difference is most marked in the zone 50°–55° N. If it is assumed that the same cyclonic distribution prevails during the autumn and that the years of abnormal temperature at Grimsö truly correspond to the exceptionally abnormal ice years for which the precipitation was computed, it might be said that, since Scotland and the Baltic coast stations are located in the zone between 55°–60° N. where the increase in the number of cyclones is 13, while southeast England lies in the zone 50°–55° N. where the increase is 30, we should expect for the latter region a greater increase in precipitation than for the Baltic coast and Scotland.

The variation in spring rainfall at the Norwegian stations latitude 61°–67° N. can likewise be explained from the pressure distribution. In the spring of heavy ice years a deep low lies off the northern coast of Norway (see fig. 5) which would probably cause heavier rainfall at these stations than in the spring of light ice years, when this low is absent.

Considering the rainfall distribution for Ireland and England in April of the same years, we find a negative departure in E+ years and a positive value in E− years. A comparison of the two spring pressure maps for heavy and light ice years, shows a smaller pressure gradient over Ireland and England in E+ years. A decrease in westerlies and consequently orographic rainfall, may therefore be expected in heavy ice years.

In another investigation, Wiese (35) sought a relationship between rainfall in Russia and ice in the Barents Sea. Reference was made to his previous results showing an apparent relationship between the state of ice in the Barents Sea and the cyclonic activity in Europe during the month of May, indicating during heavy ice years a movement of cyclones from the Black Sea north-northeastward between meridians 25°–45° E. An attempt was therefore made to see whether there is an excess of rainfall in April and May in the region lying somewhat east of this zone during years when ice is heavy in the Barents Sea. To determine the existence of this relationship, the correlation method was used. The period investigated was 1895–1916, or 22 years, giving a standard error of 0.22 for each of the coefficients. The April and May rainfall were correlated with the mean area of ice in Barents Sea during May to August and with the mean area of ice during May to June. The results obtained were:

	April rainfall	May rainfall
Area of ice:		
May–August	r=0.59	r=0.40
May–June	r=0.60	r=0.39

For the northern and middle sections of the region (the region was divided into three sections) similar correlations gave respectively r=0.71 and r=0.68, for April and May combined.[6]

[6] A test of the relationships indicated by the correlation coefficients was made for the subsequent 8 years, 1917–24. This limited test showed no cases of sharp divergence in relationships. The 1920 and 1921 droughts are in full agreement with the unusually small amount of ice in Barents Sea in these years.

An increased atmospheric circulation should also mean, according to Wiese, an increase in pressure gradient between the Bahamas and the Azores. Therefore we should expect heavier rainfall in the Bahamas with light ice conditions. The correlation coefficient between Barents Sea ice (May) and rainfall at Nassau during May to October (period of abundant rains) gave $r = -0.48$ (S. E. $= 0.22$) (1896–1916).

To sum up: It appears that in heavy, as compared with light, ice years in the Greenland Sea, heavier rainfall is experienced, in the spring, in Norway, between 61°–67° N. and generally in the autumn in Scotland, on the Baltic coast, and in southeast England, while lighter rainfall is recorded, in April, over Ireland and England between latitude 52°–54° N.

With Barents Sea ice there is a tendency for heavier rainfall in southeastern Russia in April and May but lighter rainfall at Nassau during May to October with much ice.

2. SOUTHERN HEMISPHERE

The results described above concern relationships involving ice in the northern North Atlantic and in the neighboring Arctic Ocean to 70° E. The ice in the Arctic waters facing Siberia and the American Continent has never been treated in a similar fashion, as far as I am able to learn.[7] The reason is probably the lack of adequate and systematic information. For the same reason the variation of ice in the southern hemisphere, with the exception of the South Orkneys region, has not figured in any investigation concerning the weather.

In a series of papers (36, 37, 38, 39) which appeared early in the century, R. C. Mossman presented the results of an intensive but very limited study of the meteorology of southern South America and neighboring regions, including the portion of the Antarctic which faces it. The lack of extensive data and the shortness of the period for which ice and other observations were available made it impossible for Mossman and later for Walker (40, 41) to treat the relationships in the manner of the investigators whose results have been discussed here.

A preliminary summary of the results obtained by Mossman together with the conclusions drawn by him appeared in 1910 (38). At that time he stated:

Variations in the northern limit of the pack ice in the Weddell Sea, and probably also in the sub-Antarctic waters of the South Pacific, are intimately associated with the great atmospheric movements which control the weather in South America.

In 1916 Mossman (43) wrote:

The path of cyclonic storms * * * across the southern portion of the South American continent appears to move in harmony with the northern limits of the ice belt, so that when ice is far north the cyclonic track is also north but when the ice belt retreats to the south the cyclones also pass to the south.

The evidence upon which the above assertions were made is extremely sketchy. It consists, in the main, of winter pressure, temperature and wind observations at a few isolated points, but of rainfall over a fairly large area. Even if we allow Mossman's assumption that evidence is representative it would still appear that there is little consistency in the results with reference to the ice variate. Of the four winters for which a southward shift of the high pressure belt is claimed, two, 1903 and 1904, were characterised by "close" ice and two, 1908 and 1910, by "open" ice.

It is inferred from Mossman's and from Walker's

[7] Okada (42) and Walker refer to several papers (in Japanese) in which an attempt was made to investigate the effect of ice in the Bering Sea on the following winter temperature in Japan. It may be inferred from their statements that these attempts were inadequate.

writings that when the winter at the South Orkneys is "very close," as in 1904, or "very open," as in 1908, a realignment of pressure fields and the phenomena associated with it is to be expected. It would appear that these years are characterised by opposite meteorological and ice conditions. However, the winters of 1905, 1906, and 1907 were also characterised by "very close" ice, yet for these no claim was made similar to that for 1904.

In a paper written in 1917 but published in 1923 (12) Mossman presented the results of a study of rainfall in South America and contemporary ice conditions at the South Orkneys. The period is 1903–12. Comparing the rainfall with the normal he obtained the following percentage deviations of rainfall for some of the more representative areas considered.

Season	Summer		Autumn		Winter		Spring	
Ice conditions	Open	Close	Open	Close	Open	Close	Open	Close
	Pct.	*Pct.*	*Pct.*	*Pct.*	*Pct.*	*Pct.*	*Pct.*	*Pct.*
Southern Chile (1 station)	3	0	9	10	20	−9	8	−5
Northern Chile (3 stations)	12	−19	14	−14	−41	9	−8	5
Central Argentina (41 stations)	13	−10	32	−32	−16	8	5	−3
Sao Paulo coast (3 stations)	10	−9	−16	16	−4	1	1	−1
Sao Paulo inland (10 stations)	6	−6	3	−4	−6	4	−2	1

No explanation for the manner of variation in rainfall, as given in the table, was suggested by the author.

3. DISCUSSION

Tentative explanations for some of the relationships indicated from the results presented above were suggested by a number of investigators, but notably by Wiese, and by Brooks and Quennell. In the following lines I have attempted to present the essence of these explanations and at the same time enlarge on them.

The discussion is limited to relationships involving ice in the Greenland Sea. The selection of this variate was made desirable by the fact that, on the whole, the most adequate and "significant" ice observations so far, appear to be from that region. Further it was deemed sufficient for the purpose of illustration to limit the discussion to the basic element, pressure distribution. Wiese's maps (fig. 5) giving the mean spring, summer, and fall pressure distribution for all the heavy and light ice years and the correlations obtained by Brooks and Quennell were chosen for this purpose.

From a consideration of the maps it would appear that the following takes place: In heavy as compared with light ice years pressure is higher over the immediate ice region in the spring. With the displacement of the high pressure area southeastward the belt of low pressure and centers of high pressure to the south gradually assume a more southerly position.

It may be noted that the direction in which the high-pressure field over Greenland region is displaced is probably determined to some extent by the ice distribution. The new boundary extends several hundred miles in the direction in which ice conditions are above normal, roughly parallel to the old one. The effect of the increase in the amount of ice, as represented by an intensification and extension of the high-pressure field, will probably be limited to the new ice boundary. This holds at the beginning. As the ice begins to drift southward and eastward the region of high pressure embraces an even wider area. The low-pressure belt partly in consequence of the changes in the high-pressure field to the north and partly as a result of the general decrease in intensity of circu-

lation, will also be displaced southeastward and tend to fill in.

We may now reconsider some of the more obvious effects which may be brought about by the changes in the configuration, position, and intensity of the pressure centers ducussed above. Assuming for the moment that the lowering of pressure over the British Isles implies an increase in cyclonic activity over that area we would expect the mean cyclonic path to suffer a greater displacement in the autumn than in the summer but to suffer none in the spring. Obviously, the sole fact that the pressure is lower over Great Britain in summer and autumn after a heavy than after a light ice season is not an indication of a greater cyclonic acitivity there. The pressure over the British Isles, however, using the same example, is determined to a large extent by the configuration of the general pressure field over the North Atlantic. This field, we saw, is modified after a heavy ice season, in such a way that the high pressure field in the north is displaced southward and along with it the low pressure field south of it. Thus the lowering of pressure during the summer and autumn must be associated in the main with a displacement of the belt of low pressure and hence the mean cyclonic path southward.

It may be said that one would expect to find the sort of distribution of the latitudinal difference of mean cyclonic paths between heavy ice and light ice years, as was shown earlier on the basis of the following considerations. The ice season in Greenland Sea is usually confined to the last 2 months of spring and the first 2 months of summer (April to July). The maximum effect of ice on the water temperature would be expected during the height of the ice season or, because of a certain persistence tendency of the ice and the cold water which forms as the ice melts, at the end of the season. This appears from a correlation of the ice area in Greenland Sea during April and May and the mean monthly temperature of water at Papey, East Iceland. High values were obtained beginning with May but the highest were for June and July. On the other hand the maximum departure in the water temperature which is brought about by the melting of the ice and transport of the cold northern water southward, because of the inertia of hydrological processes, may well be expected to occur over a period extending beyond the actual ice season. This appears to a certain extent from the fact that the persistence tendency of the water temperatures at Papey is greatest for the period June to September, which is to be expected if we admit a continuous supply of water from the same source.

Correlation coefficients, monthly sea-surface temperature at Papey with that of the following month (1876–1915) (S. E.=0.16)

Jan.-Feb.	Feb.-Mar.	Mar.-Apr.	Apr.-May	May-June	June-July
r= 0.61	0.61	0.81	0.76	0.90	0.84

July-Aug.	Aug.-Sept.	Sept.-Oct.	Oct.-Nov.	Nov.-Dec.	Dec.-Jan.
r= 0.86	0.89	0.68	0.69	0.65	0.70

The influence of the Greenland Sea ice on the temperature of the water to the east and of the cold water carried down from the north might be expected to be felt progressively later as the cold water carried down by the East Iceland current spreads in that direction. This was indicated to some extent from a correlation of the temperature at Papey in June with that at Thorshavn, Faroes Islands, and Ona, Norway, for the 3 summer months (31). The highest values, though very small, were found for Thorshavn in July and Ona in August.

The effect of the gradually spreading relatively colder water southward and eastward in years with heavy ice may be the cause of the progressive displacement of the

mean position of the cyclonic path over the North Atlantic which was indicated earlier. It is thereby assumed that the belt of cyclonic activity which lies along the zone of interaction between the warm Atlantic water and the cold polar water suffers a displacement as the zone of interacting waters is displaced.

It was assumed that the effect of a greater amount of melted ice and cold northern water, because of the inertia of hydrological processes, appears some time after the ice has disappeared and is therefore most pronounced in the autumn. On the other hand the effect of the exposure of atmosphere to the ice directly is most telling in the spring and early summer. In the first instance, one of the results of a heavy ice season appeared to be a displacement of the mean cyclonic path southward in the summer and especially in the autumn, as well as an increase in the cyclonic frequency south of latitude 60°, together with a decrease north of latitude 65°, in December to January.

In the second instance a large amount of ice appeared to be accompanied by a lowering of temperature and rise of pressure during the spring and early summer. Therefore, though at this season there may be assumed to be no shift of the low pressure field southward, that is to say, the mean front remains in the same position, there is a more frequent outbreak of cold air masses. The climate of Great Britian, we recall, appears to be less maritime in the spring during a heavy than during a light ice season.

The above interpretation of the role of polar ice in the atmospheric circulation is probably but a single phase of a more general and inclusive interpretation of the process involved, namely that the variation in ice is the result of the character of the preceding state of the general circulation, and that the correlation between ice and subsequent weather, as indicated in its general features by the southward displacement of storm tracks during the late summer, autumn and early winter is due to the state of atmospheric and oceanic circulation preceding the ice season and to the reciprocating effect which the ice and cold water exert on the atmospheric circulation.

Probably what takes place is about as follows: Preceding a heavy ice season we have a period of marked inactivity in the general circulation and consequent coldness of ocean and atmosphere, followed by increase in ice and high pressure in the North. The accompanying pressure distribution favors northerly winds as indicated by the above normal west-east pressure gradient from Iceland to northern Norway. The consequent southward displacement of the ice and cold water explains the heavy ice season in the Greenland Sea and the subnormal water temperatures extending still farther south. The resulting shift of the zone of maximum latitudinal temperature contrasts southward probably explains the more southerly position of maximum cyclonic activity as represented by the storm tracks during late summer, autumn and early winter of heavy ice years.

4. APPLICATION TO WEATHER ANALYSIS AND FORECASTING

Past weather has been analyzed by Brooks and by Wiese as a preliminary step to forecasting. Brooks (44) attempted to analyse the causes of pressure distribution favorable to wet seasons in the British Isles. He treated a total of 14 cases, which occurred over a period of about 50 years. Two general types of wet seasons were distinguished, one caused by cyclonic, the other by orographical rainfall.

Since there is a tendency for depressions to follow more southerly tracks in years of much ice, especially in autumn

and winter following the ice season, "we should expect wet seasons in the British Isles (of the cyclonic type) to occur most frequently in years with much ice in the east Greenland Sea. Table I (not reproduced here) shows that this variable exceeded its normal value in 8 out of 13 instances." With regard to the orographical type "this might be expected when pressure in Iceland is low (large pressure gradient over the British Isles) or when the Iceland ice is below normal, and especially in the spring of ice-poor years."

He concludes "that wet seasons in which the rainfall distribution is of the orographical type seem to be mainly dependent on the occurrence of three factors * * * (one of which is) * * * the almost complete absence of ice during the wet season * * * For a wet season of the cyclonic type favorable conditions appear to be * * * a large amount of ice off Iceland during the preceding winter and spring; for the wet seasons the relationships found are not yet sufficiently precise for forecasting purposes; * * * the complex effects associated with Arctic ice still need a great deal of research to elucidate them" (44).

The probability that the relationship between November to January pressure and Arctic ice during the preceding spring and summer (approximately), is real, led Brooks and Quennell (4) to investigate it more closely. He computed partial correlation coefficients between the ice conditions in four regions, Iceland (December to June), Greenland Sea (April to June), Barents Sea (April to June) and Kara Sea (August) on one hand, and Stykkisholm, Vardo, Valentia and Ponta Delgada on the other. A uniform period 1895–1925 was employed throughout. The partial correlation coefficients of the third order are given in table 8 which is reproduced here from their paper.

TABLE 8.—*Partial correlation coefficients* (r) *and regression coefficients* (b) *with Pressures in November to January*

	Stykkisholm		Vardo		Valentia		Ponta Delgada	
	r	b	r	b	r	b	r	b
Iceland ice (December to June)	−0.21	0.028	0.28	0.033	−0.51	−0.062	−0.05	−0.004
Greenland Sea ice (April to June)	.23	.97	.25	.92	.08	.27	.10	.23
Barents Sea ice (April to June)	.29	.82	−.02	−.05	.04	.08	.27	.41
Kara Sea ice (August)	−.10	−.63	−.02	−.10	−.08	−.41	−.13	−.46

It was previously noted that Wiese found an apparent relationship between ice in the Barents Sea and more or less contemporary rainfall in southeastern Russia. In other investigations Wiese and others showed that ice in Barents Sea tends to persist from year to year. In view of the relationships indicated between ice and contemporary rainfall, the possibility was suggested of a relationship between rainfall and ice in the preceding season. Correlating rainfall during April to May in percent of normal with ice in the Barents Sea in the preceding May to June in 1,000 square kilometers Wiese obtained (35) r= 0.52 (S. E.=0.22). The values of the April to May rainfall computed with the aid of the regression equation employing the above elements expressed in percentages of normal were then compared with the actual values similarly expressed and for the 21 years which were considered. The percentage of cases having the same sign of departure was found to be 90 percent.

To obtain a real test of the relationship Wiese employed the above formula and another formula for May

rainfall for test forecasting (33, 45). The computed values together with the observed values are given below:

Comparison of observed with forecast values of rainfall in southeastern Russia expressed in percentages of normal

	Forecast	Observed
1924, May	73	79
1925, April–May	93	88
1926, April–May	72	92
1927, April–May	111	121
1928, April–May	104	135
1929, April–May	71	89
1929, May	65	55

B. Relations between sea ice and world weather

1. GENERAL CIRCULATION

In considering relationships between ice and world weather Wiese assumed that ice reflects the intensity of the general atmospheric circulation; that the circulation is weaker with heavy than with light ice conditions. A certain amount of evidence was offered to show that insofar as the circulation over the North Atlantic and Europe is concerned (the only regions considered) such a tendency exists.

In presenting that hypothesis it was contended that the effect of a diminution of intensity of atmospheric circulation is a greater formation of ice. However, the greater amount of ice favors a higher pressure in polar regions and therefore a decrease in meridional pressure gradients, and hence a further weakening of the circulation. Thus a growth of ice caused by an incipient weakening of the circulation promotes in turn a continuation of this trend, or one aids the other. On the other hand, the presence of a larger amount of ice, in other words a southward displacement of ice and cold water, implies a greater thermal gradient between polar and equatorial latitudes and consequently an increased circulation. The inherent "contradiction" of the above state of affairs was first pointed out by Simpson (46). However, it was inferred by Brooks (29) that the opposing tendencies may not be balanced, in which case the circulation may be weaker or stronger for one period or another.

If there is assumed an excess of ice and a weaker circulation, it is apparent that the established process cannot go on indefinitely because an element is immediately introduced which opposes this tendency and finally becomes the dominating factor, breaking up the established chain. When this happens we have the reverse process at work. The circulation is intensified and, because of the stored-up energy, becomes above normal.

Thus opposing forces are at work seeking to establish a balance, which is never reached in fact. This explains the existence of the apparent contradictions, a diminished circulation associated with an excess of ice and accompanied by an ever increasing temperature gradient and, conversely, a more intense circulation associated with a deficiency of ice and accompanied by an ever diminishing temperature gradient.

The necessary implication of the foregoing is that there must exist some sort of an oscillation in the variation of amount of ice and the atmospheric circulation. To be sure it could not be periodic, nor very definite, since the forces produced by thermal and pressure gradients are probably never balanced.

It would appear that one could find a reflection of these oscillations much more readily in ice, because of

its greater staying qualities and confinement to relatively limited regions, than in the atmosphere.

Several investigations show a definite tendency toward a "periodic" variation in ice conditions. Meinardus (10) who investigated the variation in ice at Iceland over a period of more than 100 years found an average 4½-year period. Wiese (2) showed that the ice regime in the Greenland Sea also shows a similar variation. Brooks (47) who analyzed by means of a periodogram Meinardus' data augmented by subsequent observations found the period at Iceland to be 4.75 years. A similar period is also indicated in the variation of ice conditions off the North Siberian coast between Kolyma, 160° E., and Bering Strait (48).

It would appear from the above considerations that, to a certain extent, ice in polar regions is indicative of the state of the atmospheric circulation. Direct evidence that this ice is related to world weather is practically nonexistent. Little effort has been directed along these lines and the evidence obtained is inadequate to enable one to draw definite conclusions. Wiese (49) sought a relationship between polar ice and rainfall in equatorial regions on the assumption that an increased circulation is associated with heavier precipitation, due to greater convection.

Because of lack of extensive rainfall observations in equatorial Africa, Wiese chose the mean level of Lake Victoria as representative of the relative value of precipitation in a part of that region. For South America the choice of the actual values of precipitation was made possible. As an indication of ice conditions in polar regions, Wiese took the ice regime in Barents Sea. A correlation of May to July ice in the Barents Sea (1896–1916) with contemporary values of the mean level of Lake Victoria gave a coefficient of −0.62 (S. E.=0.22).

It was further assumed that the process of a diminution or intensification of circulation is reflected first in the precipitation of equatorial regions. To check this, Wiese correlated ice in the Barents Sea, May to August, with the mean value of level of Lake Victoria during January to April. The value of the correlation coefficient was slightly higher, −0.66 (S. E.=0.22). A correlation of May and June ice with the mean value of the January and February level gave $r=-0.72$ (S. E.=0.22). Ice in Barents Sea was also correlated with the annual precipitation at 10 stations in equatorial South America (1896–1916). The value of r was −0.61 (S. E.=0.22).

Wiese also sought to find a relationship between ice in the Antarctic and the two elements discussed above. Because of the nonexistence of adequate ice observations Wiese took the pressure difference between Punta Arenas and Cape Pembroke, which indicates the strength of the south winds and therefore might possibly be considered as a measure of the amount of ice brought northward. Correlating the pressure difference for February to April for the 20 years with mean level of Lake Victoria for January to April he obtained a coefficient of −0.46 (S. E.=0.23). A correlation of pressure difference with South American rainfall gave $r=-0.24$ (S. E.=0.21).

2. WALKER'S OSCILLATIONS

The three oscillations conceived by Walker: namely, the North Atlantic, North Pacific, and southern may be defined as statistically derived systems of pressure, temperature, precipitation, and other elements which were obtained by correlating the meteorological elements at a large number of stations with one another and

selecting several elements which showed a marked mutual relationship. The elements were then incorporated in a formula in which each was given a certain weight which was determined somewhat arbitrarily from the value of the coefficient.

Though several investigations have indicated relationships between ice conditions in polar regions and some of the elements which comprise the systems it would be too much to say that relationships would be expected also with the systems as a whole. The correlations carried out between ice and the three oscillations gave (50, 51) a significant negative coefficient apparently only in case of the North Atlantic oscillation March to May and June to August with ice in the Barents Sea of the same year, and this may be due to presence in this particular system of a number of factors which would be expected, from other considerations, to show a relationship with ice conditions. No correlation of the spring and summer North Atlantic oscillations was carried out with ice in the Greenland Sea.

With the North Atlantic oscillation, in the following (52) December to February no relationship was indicated with Barents Sea ice April to July (45 years) and Newfoundland ice, March to July (27 years), and March to August (38 years). Similarly, no relationship was indicated from a correlation between the southern oscillation, June to August, December to February and prior (2 quarters) South Orkneys ice (13–24 years).

C. Miscellaneous relationships

In this section will be considered attempts to find relationships between ice and meteorological variates without reference to any particular hypothesis. One might expect that variates which are related to variates treated in sections A and B would show a relationship here as well. However, the elements correlated with ice, with one exception, did not figure in either of these sections.

Mossman in his investigation of Indian monsoon rainfall in relation to South American weather, states ((12) p. 167):

> With regard to monsoon rains a comparison of the departures from the normal for the southern autumn (March to May) and winter (June to August) conditions shows no definite relation with ice conditions at the South Orkneys during close and open seasons.

Wiese attempted to show (5, 6) that the states of the atmospheric circulations in the Northern and Southern Hemispheres tend to be analogous to each other, and for this reason, also the state of polar ice and the distribution of pressure, temperature, etc. To obtain an idea of whether such a relationship exists Wiese compared observations of ice at the South Orkneys, available for the period 1903–12, with those from Barents Sea for the same period. The comparison was limited, in case of the South Orkneys, to March to May, (southern autumn) when possible local influences on the formation of ice apparently are least, and in the case of Barents Sea to May to August. From the table below it appears that years with heavy ice at the South Orkneys are characterized with a positive departure in the Barents Sea and conversely, years with light ice conditions show a negative departure.

The fact that not a single exception to this rule was found led Wiese to presume that the relationship, despite the short interval of time involved is not accidental. The analogy between the ice regime of the Southern and Northern Hemispheres is however less successful than it appears. In the comparison of ice observations at the South Orkneys with those of Barents Sea, Wiese's choice

of the autumn season was motivated by the desire to eliminate as much as possible local influences on ice formation. While it is admitted that in the autumn these influences are least it is perhaps significant that when observations of the summer season, an almost equally equable season, were used in the comparison, as was done by Walker (32) and by myself, the coincidence which was found was very poor.

Comparison of ice at South Orkneys, designated as "open" or "close," with ice in Barents Sea in departures (1,000 sq. km.) from normal (22 years)

"OPEN" YEARS

	1904	1905	1906	1907	1908
Barents Sea	−158	−165	−61	−130	−121

"CLOSE" YEARS

	1903	1909	1910	1911	1912
Barents Sea	+88	+82	+17	+97	+176

Along with an increase in the area of ice in polar regions due to a diminution in intensity of the atmospheric circulation there should also occur, it was reasonably assumed, a rise in pressure in these regions. To ascertain the existence of such a relationship Wiese correlated the observations of mean monthly pressure at McMurdo Sound, available for the periods February 1902 to January 1904 and January 1911 to December 1912, with the mean pressure from Gjesvaer, 71° N. 25° E., the most northerly station, apparently, for which records for any length of time were available. A positive correlation coefficient of 0.42 (S. E.=0.26) was found for the period from November to the second February, or 16 months. A correlation of mean monthly pressure values between another Arctic station, on the northeast coast of Greenland, (75° N.) and McMurdo Sound, for the period December 1911 to July 1912 (19 months) gave a value of 0.54 (S. E.=0.24.).

The very short period covered by the records involved here precludes comment on the results. It occurs to me that since seasonal and geographical factors operate most distinctly against a general uniformity of circulation, it is doubtful if a marked analogy in ice and pressure variations between the two hemispheres could be found.

D. Relations between icebergs and weather

The icebergs that were considered in the limited investigations are those observed mainly in the South Atlantic Ocean. Walker (53) investigated the effect on Indian monsoon rainfall of icebergs in the Southern Ocean, over the periods 1885–1912 and 1869–1912.

It will be seen that in the South Indian Ocean (20° E. to 149° E.) the number of occasions on which ice is recorded may be described as small from 1888 to 1892, large in 1894, and very large from 1895 to 1897, small from 1898 to 1902, large in 1904, small in 1905 and 1906, and large in 1909. Thus ice was abundant in 1894 and 1909 when our rains were good, as well as in 1895, 1896, and 1904, when our monsoon was deficient, and in a similar way ice was scanty in 1889, 1890, 1892, 1898, and 1900, when our rainfall was above the average, as well as in 1891, 1899, 1901, and 1905, when the monsoon failed. It does not therefore appear that the ice (icebergs) in the South Indian Ocean exercises a material influence on the Indian monsoon.

The data for the South Atlantic Ocean are, however, more promising in this respect * * * it appears that the quantities of ice were small from 1885 to 1891, were large in 1892, very large in 1893,

and did not again become really large before 1906. In that year and in 1908, very many observations are indicated, while there are many in 1910 * * * it may be gathered that icebergs were extremely numerous from April to October 1892, from December 1892 to June 1893, from September 1893 to January 1894 and in 1908; * * * fairly numerous * * * 1869, 1875, 1879, and in 1906. Now if we turn to the data of annual pressure at Santiago, Buenos Aires and Cordoba * * * we find that in the 3 years mostly affected, 1892, 1893, and 1908, the mean of the pressure departures of these places was 0.47 mm., 0.77 mm., and 0.51 mm. Further in 1869, 1875, 1879, and 1906 the mean departures were 0.14 mm., 0.32 mm., −0.03 mm. and 0.05 mm., respectively. The number of years for which information is available is not large enough to justify a definite conclusion; but the data emphatically suggest that years of much ice off South America tend to be years of high pressure in the Argentine Republic and Chile. We should accordingly expect them to be years of good rainfall in India; and it is interesting to see that in the monsoons chiefly affected, those of 1892, 1893, and 1908, there were excesses of 4.93 inches, 3.64 inches, and 2.10 inches; while in the years 1869, 1870, 1875, 1879 * * * and 1906 less affected, the departures were −0.11 inch, 1.42 inches, 4.41 inches, 2.28 inches * * * and −0.11 inch. The mean departure for the first group is 3.56 inches and of the second group 1.58 inches.

It is perhaps worth while to point out in connection with the above that the largest number of icebergs recorded was 306, in 1906, of which 271 were in the South Atlantic. It occurs to me, in the light of other information, that the ratio of actually observed icebergs to those that are released is negligibly small.

Walker also correlated (52) the southern oscillation, December to February and June to August with contemporary and preceding (6 months) South Pacific icebergs. The coefficients were negligible.

III. SUMMARY OF THE MAIN RESULTS AND REMARKS

The main results presented here indicate a contemporary and subsequent relationship between ice in the polar seas of the Northern Hemisphere and the general circulation as indicated by the pressure distribution as well as the phenomena associated with it (temperature, cyclonic activity, rainfall) over the North Atlantic and Europe.

With a heavy ice season in the Greenland Sea (approximately April to July) pressure is generally higher in the neighborhood of the polar seas but lower in northern Scandinavia, the Norwegian Sea, and to a smaller extent generally elsewhere in Europe and the North Atlantic. Following the heavy ice season in autumn, pressure continues high in the neighborhood of the polar seas and is low over western Europe, especially the British Isles, northern France, etc.

The variation in pressure at various points, and in the pressure difference between them, appears to be brought about through changes in the intensity of pressure centers and through their displacements, the general trend being, in heavy ice years, toward a filling up of the Icelandic LOW, a flattening of the Azores HIGH, a retreat of the westerly extension of the Siberian HIGH, and shift of the pressure centers equatorward. This appears to be accompanied by a diminution of cyclonic frequency and a southward displacement of the mean cyclonic path over the North Atlantic, in the summer, autumn, and early winter following the heavy ice season, which apparently produces the heavier rainfall in the regions affected, notably the British Isles [3] and the Baltic coast. A sub-

[3] In a discussion of Wiese's results as they pertain to periods of excessive rainfall over the British Isles the following statement by C. E. P. Brooks is quoted: "In spring and summer of a year with much ice, pressure tends to be above normal near Iceland, diminishing the force of our westerly winds. In spring the high pressure tends to spread over the British Isles and Scandinavia giving us a fine, though rather cold season; but for the remainder of the year pressure over the British Isles tends to be below normal, and the weather to be wet and stormy. This is especially noticeable in the late autumn and early winter; and some of our most disagreeable wet seasons, notably 1912 and 1918, can be attributed mainly to an excess of ice near Iceland and in the Greenland Sea" (Q. J. R. Meteor. Soc. p. 137, 1930).

sequent analysis of wet periods in the British Isles indicated an association with ice conditions in the Greenland Sea.

With a heavy ice season in the Barents Sea somewhat similar relationships obtain. Rainfall is above normal in central and southeastern Russia during April and May. Attempts at relating past rainfall to ice and the forecasting of rainfall have proved moderately successful.

In connection with the results presented above, various limitations and difficulties must be pointed out. The data upon which they are based are meager. Many of the conclusions are based on studies of years in which the ice was in fairly marked defect or excess rather than on all the available data. Again, and this is especially noteworthy from the point of view of forecasting, the results are based on averages and often differ markedly in individual years. On the other hand, the various results obtained by different investigators using data for somewhat different periods have proved fairly consistent.

Since the physical basis suggested for the various relationships is still uncertain, we have no definite assurance that some of the results are dependent on the ice. *This does not diminish the value of ice as a criterion of the preceding, contemporary, or, what we are mainly interested in, subsequent weather, especially since the other factors are unknown.* On the other hand, unless the other influences are also ascertained, we shall ever be somewhat at loss as to the correct evaluation of the ice factor.

To determine finally the role of polar ice it will be necessary to elucidate a multitude of points. These may be classified broadly as (1) the manner of variation of ice, (2) the relationships between ice and the weather, (3) the physical basis underlying these relationships. The above necessarily implies a study of other factors which appear to be related to ice as well as to the weather.

In conclusion I wish to point out the advantage of employing ice as an index of the general circulation and the weather, by virtue of the persistence tendency or relative inertia to changes with which the ice is characterized, and the stabilizing effect which ice apparently has on the general circulation. We might expect that an element characterized by a certain amount of persistence tendency would not only reflect large scale and complex changes in the circulation in a simpler manner than an element which does not possess this property to a marked degree but that it would also simplify these changes through its stabilizing effect. On the other hand, any element characterized by too much inertia might be expected to be too insensitive to important changes in the circulation. Thus an element is required which is neither too elastic nor too rigid (in the above sense); and it appears that ice is one

It should be added that none of the investigations directly concerned the weather in the United States. There is good reason to believe, however, that the apparent variation with ice conditions of the positions and intensities of the Icelandic LOW and the Azores HIGH is reflected in the weather in this country. Nor did any of the investigations reviewed deal with the known variations in ice conditions off the Alaskan Peninsula and northeast Siberian coast. It seems probable that an association between the state of ice in those regions and the weather in Canada and this country may also exist.

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DISCUSSION OF SOME THEORIES ON TEMPERATURE VARIATIONS IN THE NORTH ATLANTIC OCEAN AND IN THE ATMOSPHERE

By R. B. Montgomery

The first part of this discussion deals with the work of Helland-Hansen and Nansen (1). It is not necessary for our purpose to give a detailed review of this book. I wish merely to present the general conclusions reached by the authors, together with a few examples of the evidence on which the conclusions are based. I wish also to discuss, in certain phases, the validity of these conclusions.

The first part presents Helland-Hansen and Nansen's study of the surface water and air temperatures and winds in a number of regions of the North Atlantic Ocean (see fig. 1) [1] for the years 1898–1910, especially for the months February and March.

It has been suggested by many authors that variations of North Atlantic surface temperatures, and of water and air temperatures along the northern coast of Europe, may be due to variations in the strength or temperature of the permanent ocean currents, particularly of the Gulf Stream system. The authors of the present book show that this is very unlikely. There is practically no evidence of any progression of water temperature anomalies along the paths of the ocean currents, but rather anomalies of the same sign appear simultaneously over large areas. For yearly temperatures the agreement covers the whole eastern North Atlantic between latitudes 30° and 60° or even down to 18°. (See fig. 2, for example.) Furthermore, according to the authors, the air temperature anomalies, which on the whole run parallel to those of the water, have magnitudes larger than those of the water. This is indicated in figure 3a, but in figure 3b the two magnitudes are very nearly the same. If the air temperature anomalies result directly from those of the water, the former would be expected to be smaller than the latter. Finally, the authors point out that the water along the Norwegian coast, where the temperature is known to have close associations with Scandinavian weather, is not even in part supplied by the Gulf Stream (as was supposed by Otto Pettersson).

The conclusion reached in the book is that anomalies in water and air temperatures result from anomalies in the local winds, and that the two temperature anomalies occur independently. For instance, where the winds have an abnormally large southerly component, the air temperature is raised and surface water is driven northward, or not so intensely southward, with the result that the water temperature is also raised. This phase will be discussed in more detail:

Figure 4 shows normal surface isotherms for February, and normal gradient winds for January and February as determined from surface isobars. The authors find that, in the open ocean region where there are no strong disturbing factors, the angle between gradient wind and isotherms varies only between 29° and 47°, with a mean of 39°. Assuming water temperature to be a conservative property, the motion of the surface water must follow the isotherms. Hence the authors consider this angle of 39° to be in excellent agreement with Ekman's value of 45°

for the angle between surface wind and the drift current at the surface. The first objection to this is that water temperature at the surface can hardly be sufficiently conservative to justify this reasoning. The second objection is that the authors failed to realize that, regardless of the direction of the surface current, the transport of a steady drift current in deep water is directed 90° from the surface wind. Of course a gradient current may be superposed, so that the total transport is at a smaller angle.

In plates 16–41 the authors give charts for individual months, two of which are reproduced here in figure 5. The plain figures are departures from normal for water temperature, the encircled figures for air temperature, both in tenths of a degree. The gradient winds are shown by arrows, the thin arrows being normal values. These charts show clearly that, where the winds blow more from the south than normal, in general both air and water temperatures are raised, and with northerly winds they are lowered. This is strong evidence in favor of the temperature changes being due to changes in atmospheric circulation.

In order to put this on a rough numerical basis I have studied the temperatures and wind from the region 37°–49° N. and 20°–40° W. Throughout this region the water is normally warmer than the air, the average difference for the periods involved being 0.85° C. This is approximately the region where the authors found an angle of 39° between isotherms and gradient wind, the wind blowing from warmer to colder water. From the 26 charts I have tabulated all cases where the wind vectors have magnitudes equal to or greater than normal, but where the angular deviation from normal is at least 20°. There are 19 cases where the deviations are toward southerly winds; the average water temperature anomaly is 0.38°, with all cases giving positive or zero departure; the average air temperature anomaly is 0.88°, with 16 cases positive. There are 24 cases of northerly winds; water anomaly −0.37°, 15 negative; air anomaly −1.18°, 20 negative.

It is desired to determine whether the essential effect of anomalies of wind on temperature is (1) to produce meridional displacements of the water which in turn affect air temperature, or (2) to produce meridional displacements of the air which in turn affect water temperature, or (3) to produce independent displacements of air and water. The first alternative is contradicted by the fact that the anomalies of water temperature are smaller than the corresponding ones for air. But the analysis above does not favor the third alternative over the second, and hence does not corroborate the authors' conclusion.

For the same region I have tabulated all cases where the wind vectors have approximately normal direction, but where the magnitude differs from normal. The magnitude has a positive deviation in 34 cases, for which the mean water temperature anomaly is −0.30° and the mean air temperature anomaly −0.26°. The magnitude has a negative deviation in 20 cases, for which the corre-
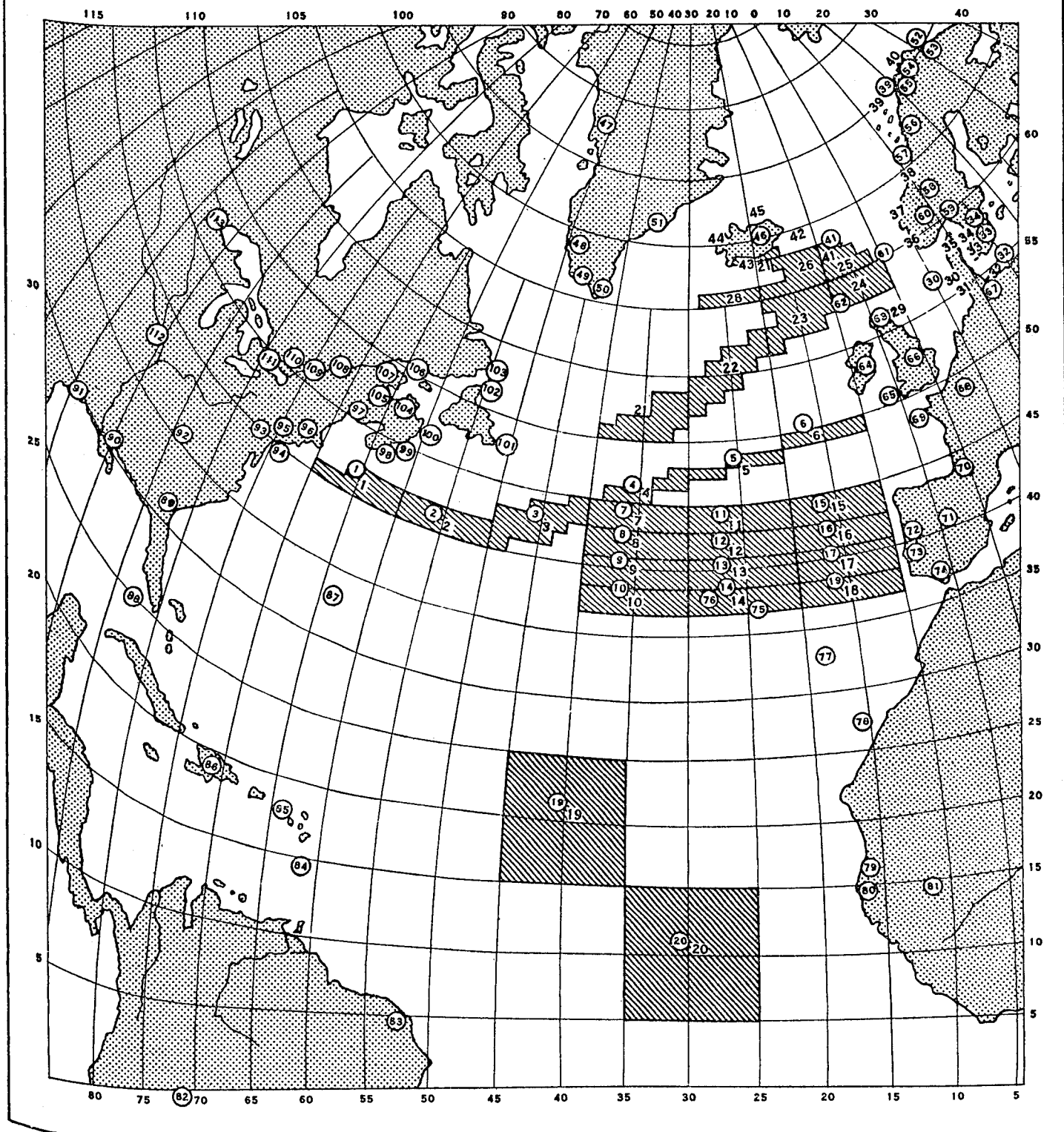
Figure 1.—Location of observations of temperatures of air (in circles) and water (plain figures).

sponding quantities are 0.06° and 0.045°. These quantities are not symmetric because the normal values are not means for the one wind direction.

If alternative (3) above is the correct one, it would be expected that winds above normal would give negative water temperature anomalies and positive air temperature anomalies, and vice versa. The values found in the preceding paragraph do not confirm this but rather favor alternative (1). Hence the evidence is conflicting and does not lead to the conclusion reached in the book that alternative (3) is the correct one.

The second part of the book deals with atmospheric variations in general, and a comparison of these with solar phenomena. The conclusions reached in this second part may be quoted from the conclusion of the book:

> The point of departure in these investigations was the wish to investigate more closely some of the yearly temperature variations in the North Atlantic Ocean. We have seen that such variations are present and that they are very considerable and extend over great regions in common. They can be ascribed in greater part to
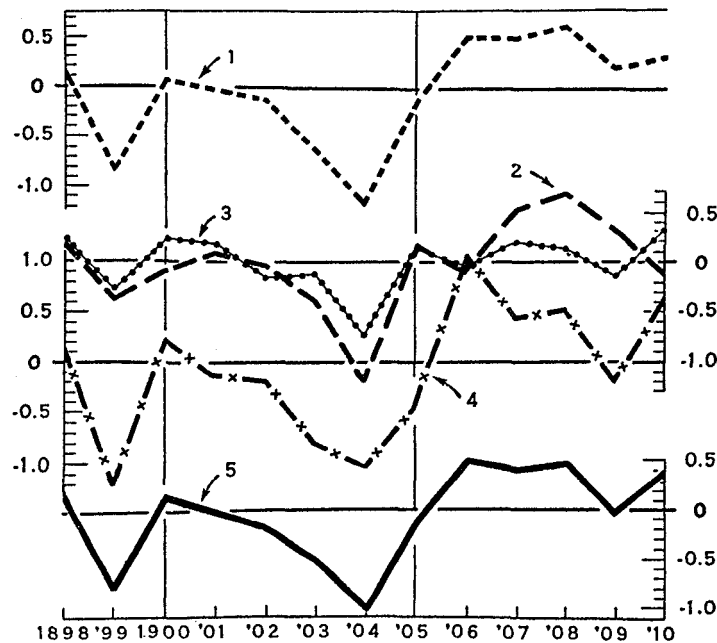

FIGURE 2.—Anomalies of ocean surface temperatures, Feb. 3 to March 4. 1. Average of six 10° longitude areas New York to the English Channel. 2. Average of the three eastern 10° longitude areas, New York to the English Channel. 3. Average of twelve 10° longitude areas, Portugal to Azores. 4. Average of the three western 10° longitude areas, New York to the English Channel. 5. Average of curves (2+3)/2 and 4.

the action of the air pressure distribution, that is to say, the winds. In order to understand the occurrence and the nature of the variations, meteorological variations must therefore be closely studied. These can be understood only when the atmosphere as a whole is investigated, and we are therefore led to make a very wide investigation.

Hithereto these extensive investigations have shown us that different groups of regions vary intact in a definite direction, while another group of regions varies in an opposite sense, and that again still other regions show transition phenomena, partly on account of phase displacements and partly on account of mixed relationships to the primary groups. All this gives us a variegated picture of the meteorological fluctuations, but out of this same variegated picture we find also by a proper analysis the influence of the variations in the solar activity which in all probability make themselves felt first in the higher layers of the atmosphere and thereby produce disturbances which again introduce changes in the lower layers. Such dynamic changes will take different courses in respect to the temperature, cloudiness, precipitation, etc., at different stations of the earth. But it seems possible by a thorough evaluation of available observational material to work out sure and general rules to cover the phenomena.

The authors arrive at the importance and the mechanism of solar radiation as follows. Atmospheric phenomena show a parallelism to solar phenomena. (The method used was a visual comparison of many smoothed curves, see for example fig. 6.) But the first effect of solar changes is not found to be a change in surface air temperatures, but rather surface meteorological phenomena result directly from the general atmospheric circulation. Hence the direct effect of solar changes must take place at some level above the surface.

This needs modification in view of Simpson's very important and masterful paper of 1928 (2). Assuming that the spectral distribution of solar radiation does not change, Simpson finds that radiation balance for the earth may be maintained during a 1-percent increase of solar radiation intensity by any one of the following mean atmospheric changes:

(a) An increase in surface temperature of 2°.
(b) An increase in stratosphere temperature of 1.5°.
(c) An increase in cloud amount of 0.01 of the area of the sky.

Simpson says: "Now there is very good reason to believe that variations in solar radiation of the order of magnitude of 1 percent have occurred in recent years, without anything like a change of 2° in mean surface temperature. On the other hand he believes it likely that an increase in solar radiation, by means of an initial increase in meridional and monsoonal temperature gradients, results in an increased atmospheric circulation which produces the slight increase in cloudiness necessary to effect radiation balance. At the same time the increased circulation can produce marked changes in weather which depend on a station's locality with respect to the circulation systems.

Simpson does not mention the following: The change in cloudiness he prescribes keeps the solar energy absorbed by the earth essentially constant in spite of a change in solar radiation. Hence it is difficult to find the source of the additional energy necessary to maintain the increased circulation. If he is correct, it means that the atmosphere as a heat engine is more efficient when the cloudiness is greater. This is probably true, due to increased condensation.

Hence Simpson's work is essentially in agreement with the conclusions of Helland-Hansen and Nansen regarding the effect of changes in solar activity, but it gives a different interpretation.

NOTE CONCERNING A PAPER BY FOLKE BERGSTEN

In spite of the conclusion of Helland-Hansen and Nansen that variations in surface air and water temperatures in the northeastern North Atlantic are not in the main caused by variations in the Gulf Stream and Labrador Current, the opposite is still considered an established fact by many writers. An example of this is a paper which recently appeared by Folke Bergsten (3).

The starting point of this paper is climatological: the great warmth of northwestern Europe and the Norwegian Sea compared with the average for the same latitude. This is immediately attributed to the Gulf Stream. Now it is true that there is a warm northward current between Iceland and Scotland (largely in the Faroe-Shetland Channel), which has a heat capacity of the same order of magnitude as the winter air transport through the same section. But it is a misnomer to call this the Gulf Stream; properly and technically the name Gulf Stream applies only to the current west of longitude 40° W. The current here in question may be considered one of the terminal branches of the Gulf Stream system,

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but it is unlikely that its variations in intensity follow those of the Gulf Stream. On the other hand it is rea-
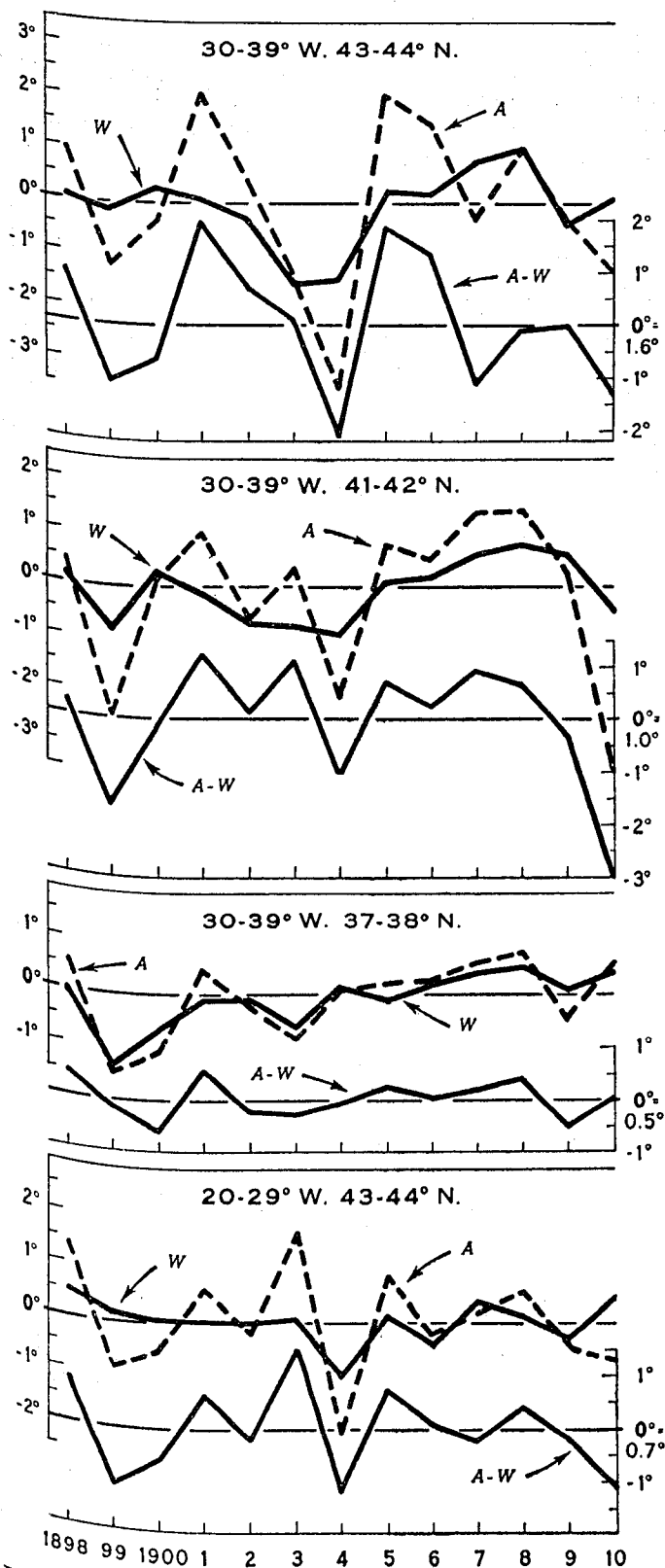

FIGURE 3a.—Air and water temperature curves and differences for four areas between Portugal and the Azores, February 3 to March 4.
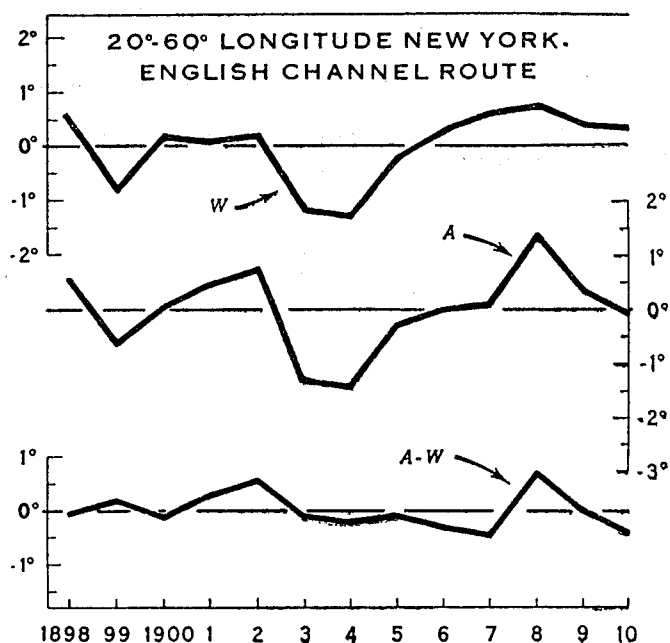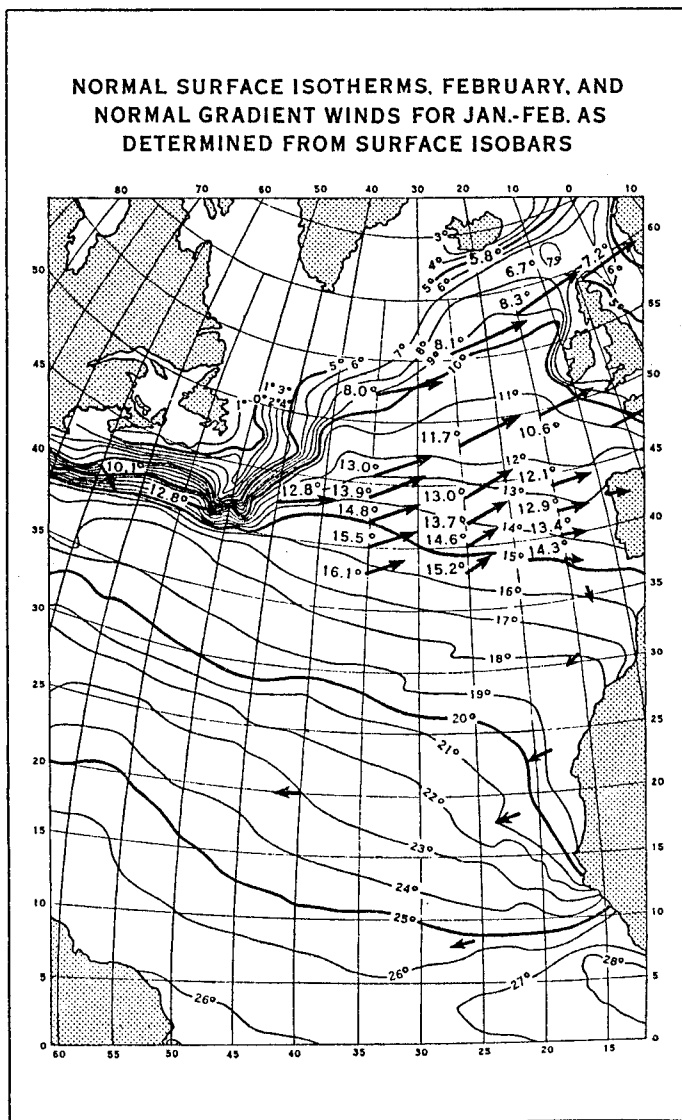

FIGURE 3b.—Air and water temperature curves and differences for the four middle 10° areas between New York and the English Channel, February to April.

FIGURE 4.

sonable to assume that its variations depend in large part on the atmospheric circulation in the region east and south of Iceland.

From this climatological starting point Bergsten proceeds on the assumption that changes in European temperature must be due to changes in the Gulf Stream. As a measure of the Gulf Stream Bergsten has carefully

DEPARTURES FROM NORMAL IN TENTHS OF A DEGREE C. FOR WATER TEMPERATURES AND AIR TEMPERATURES

FEB. 1898

MARCH 1902

30 Water temperatures   → Gradient winds
42 Air temperatures   ⇒ Normal winds

FIGURE 5.

compiled monthly surface water temperatures for the quadrangle 59°–61° N., 10°–30° W. for April to October 1900–1933. The current in question, I believe, lies about on the eastern edge of this region.

He has then correlated the yearly values (mean of the 7 months) with temperature in the following December–March at 18 land stations. Isocorrelation lines are shown in figure 7. The following are the highest coefficients:

Karesuando (northern tip of Sweden) _____ 0.30
Bodö _____ .36
Västmannö _____ .38
Thorshavn _____ .41

Hence the relation is limited to northwest Scandinavia and the stations adjacent to the quadrangle.

But there is a high correlation of December to February Scandinavian temperatures with the contemporary values of Walker's North Atlantic oscillation (4) which in turn has almost negligible correlations with the oscillation in previous quarters. Hence, unless the winter oscillation is caused by the summer water temperature and the water temperature is independent of the summer oscillation (which are disproved below), Bergsten's low correlations are to be expected.

It is tempting to make use of Bergsten's convenient and careful water temperature values. Below I have tabulated
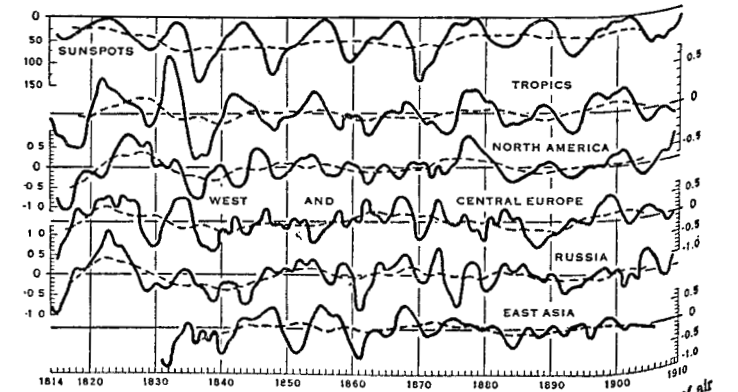

FIGURE 6.—Three-year (full line) and eleven-year (dashed line) running averages of air temperatures in several regions of the earth and sunspot numbers (inverted).

the departures from normal of the mean of his June, July, and August values, in degrees centigrade.

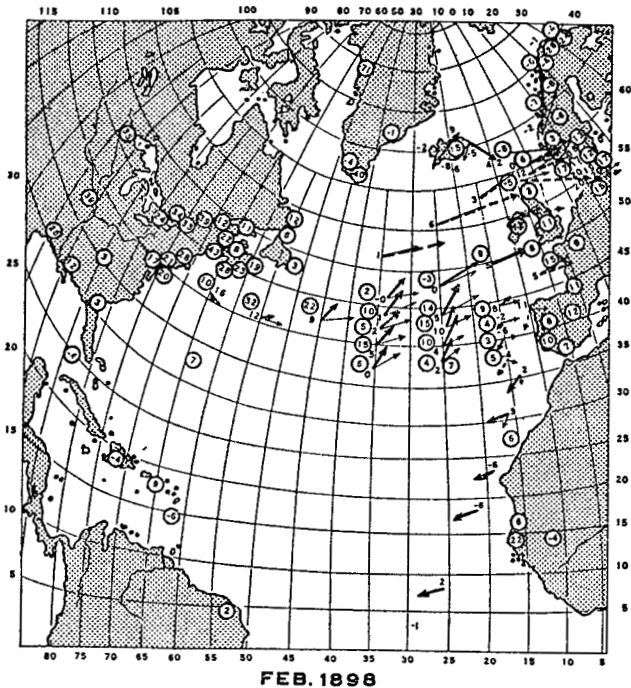|	0	1	2	3	4	5	6	7	8	9
1900	1.4	−0.4	0.4	0.1	0.4	0.0	−0.6	−0.5	−0.5	0.4
1910	.2	.0	−.1	−.4	−1.0	.5	.5	.2	−1.1	.1
1920	−.1	−.6	−.9	−.3	.2	−.5	.5	.7	.4	.4
1930	−.2	.3	.7	1.3						

Normal is 10.9°. The standard deviation is 0.58° (excluding the last 1, 2, or 3 years it is 0.54°).

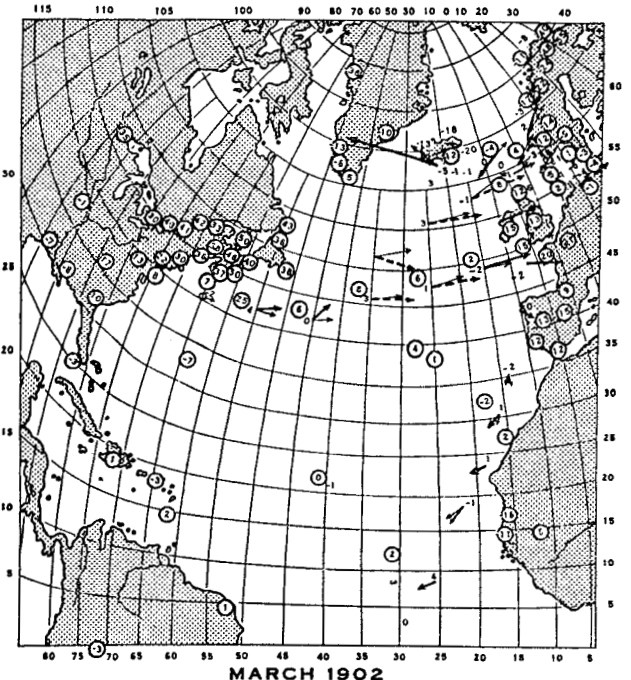The correlation coefficients of the water temperature with Walker's North Atlantic oscillation are as follows:

	S–N	D–F	M–M	J–A	S–N	D–F
North Atlantic oscillation in quarters later	−3	−2	−1	0	1	2
With water temperature June to August	−.21	−.41	−.42	−.59	−.26	−.02
Years	34	31	32	33	33	30
With North Atlantic oscillation, June to August	.16	.38	.30	1.00	.08	−.04
Years	56	56	57	-------	57	55

In the same table are reproduced some intercorrelations of the oscillation. The multiple correlation of the water

temperature with the oscillation 0, 1, 2 quarters previous is 0.66.

It is seen at once that the summer water temperature in the quadrangle has no effect on the value of the
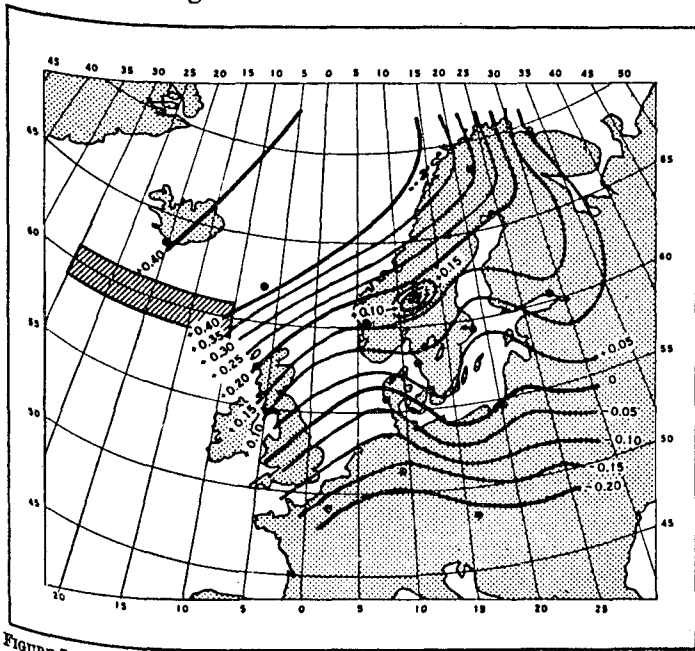

FIGURE 7.—Correlation between the summer temperature of the surface water in the shaded region and the air temperature of the following winter in the north of Europe.

oscillation in the following winter. On the other hand the oscillation in the contemporary and previous two quarters has a considerable effect on the water temperature. This is an excellent example in favor of Helland-Hansen and

Nansen's thesis that water temperature anomalies result largely from anomalies in local winds.

It should be noticed that the correlation between the oscillation and the water temperature is negative. Walker's chart 6 in "World Weather VI," showing correlations of contemporary air temperature with the June to August value of the oscillation, gives positive values in this region: Grimsey 0.46, Stykkisholm 0.44, Thorshavn 0.76. Thus, when the southwest wind is stronger than normal here in summer, the air temperature is raised and the water temperature lowered. This supports, better than any evidence given by them, Helland-Hansen and Nansen's thesis that water and air temperatures are affected independently by the wind. Of course we cannot say whether the cooling of the water is due to a drift current moving southeast, or to mixing of the surface water with subsurface water.

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(2) G. C. Simpson: Further studies in terrestrial radiation. Memoirs of the Royal Meteorological Society, 3, No. 21 (1928).

(3) Folke Bergsten: A Contribution to the knowledge of the influence of the Gulf Stream on the winter temperature of Northern Europe. Geografiska Annaler, 18, 298–307 (1936).

(4) G. T. Walker: Memoirs of the Royal Meteorological Society, 4, 53–84 (1932); 4, 119–139 (1937).

It should be mentioned that reference (1) contains a fairly complete review and bibliography of all previous literature concerning the subject matter in the title.

SUMMARY OF THE METHODS USED AT THE SCRIPPS INSTITUTION OF OCEANOGRAPHY

By R. B. Montgomery

Introduction.—Water temperature readings from the pier of the Scripps Institution at La Jolla have been taken daily since 1916. In that year Dr. G. F. McEwen started making experimental forecasts of seasonal rainfall on the basis that subnormal water temperatures in the summer are followed by relatively heavy rainfall during the following winter in the coastal region of southern California, and vice versa. The first published indication (forecast) was in 1921, and one has been issued each October since then. In each case he merely presented the evidence and stated that it was largely empirical so that, in view of the short period of the water temperatures, it could not be regarded as very reliable.

These forecasts proved successful for a few years and drew the attention of the power interests in the region. However, the method did not work perfectly (the indications were entirely wrong for 1924–25), and it became evident that this one indicator was insufficient to give reliable forecasts. Hence the power interests made it possible for the institution to expand its program in this field in 1928, beyond the limited time that McEwen himself could spend on it. Accordingly attempts were made to utilize the water temperatures in more complicated formulae for forecasting rainfall and to include air temperatures, and methods were developed for forecasting the monthly water and air temperatures three months in advance. Weather cycles were also studied and utilized in forecasting. During this time the work was carried out by Dr. A. F. Gorton, and some of the results were published under his name.

Due to the failure of the forecasts for 1929–30, outside support for the work was removed, and further extensive research was discontinued in 1933. Since then, owing to the continued interest indicated in his work, McEwen has, however, continued to apply the procedure already developed.

1. RAINFALL FORECASTS BASED ON DIRECT USE OF WATER TEMPERATURES

The seasonal rainfall figures used are those for the period from July 1 to June 30. The summer months in California are dry, the rainy season being contained between November and April; January usually has the most rain and over half the precipitation occurs in the 3 months January, February, and March. Hence the rainfall indications issued in October for the year (July to June) are predominantly for coming months.

As a physical basis for this forecasting method McEwen (7) offers the following. The North Pacific high-pressure area is most intense, necessitating an excess of air, in summer, when North America is a region of relatively low atmospheric pressure. During winter, on the other hand, there is relatively high pressure and excess of air over the continent. This necessitates a transfer of air between summer and winter from the North Pacific to North America, following the general circulation from west to east. This transfer occurs intermittently in the form of storms. "The mass of air that is thus transferred to the land during winter should be proportional to the initial mass over the ocean." From year to year the summer accumulation of air over the Pacific is subject to variations, causing proportional variations in the subsequent transfer of air to the continent. The ocean air which is transferred across the coast in this manner is moisture laden and hence capable of giving precipitation, in proportion to the annual transfer of air from the ocean to the continent. However, the mechanism for precipitating the moisture need not always be present, thus indications of a deficit of rainfall should always be fulfilled, whereas indications of an excess should not necessarily be fulfilled. During summer the pressure gradient between the Pacific HIGH and North America is associated with prevailing northwest winds along the California coast, which transport the surface water offshore (the wind drift transport being deflected 90° to the right of the wind in the Northern Hemisphere), thus producing upwelling and lowered temperatures along the coast. Thus departures from normal in the summer Pacific HIGH from year to year are attended by simultaneous departures in coastal water temperature, and by subsequent departures in rainfall.

In support of part of this argument McEwen offers the following table giving monthly averages of pressure gradient in inches per 1,000 miles from the center of the Pacific HIGH to the coast and of ocean temperature reduction below the latitudinal normal at San Luis Obispo (see also (11)):

	J	F	M	A	M	J	J	A	S	O	N	D
Pressure gradient	1.00	1.28	1.62	1.87	2.07	2.30	2.40	2.56	2.54	2.30	1.89	1.00
Ocean temperature reduction (degrees)	2.0	2.1	2.6	3.5	4.7	6.2	7.4	8.0	8.3	8.2	7.6	6.0

Later (19), McEwen states that "actually, the sea temperature is an index of a complex of conditions related to our weather." By this he evidently means that the degree of lowering of the coastal temperature depends not only on the intensity of the Pacific HIGH, but also on local conditions such as cloudiness and stirring due to wind.

There are further criticisms of McEwen's scheme which seem obvious. If we take his scheme literally, that is to assume that the air which crosses California from the west during the fall and early winter consists exclusively of the excess air which was stored up in the Pacific HIGH during the summer, there are the following three criticisms. This scheme could be roughly checked by calculation and it seems probable that much more air crosses the coast than could have been stored in the HIGH. Again, if he is correct, then the transport should cease in midwinter when pressure is lowest over the ocean and highest over the land; but the rainy season continues into April. Again, if he is correct, the yearly transport should depend not only on the summer intensity of the Pacific HIGH, but on the differences in summer and following winter between the Pacific HIGH and the pressure over the continent. If we

do not take his scheme literally, it loses its exact physical basis and reduces to saying that the Pacific HIGH seems to be a likely indicator for subsequent rainfall, although the exact relation between the two is unknown.

McEwen (13) also mentions another and more plausible explanation of the relation between coastal water temperatures and following rainfall.

* * * the oceanic circulation about the Pacific HIGH carries cold inshore water, forming the California current, into the eastern equatorial region south of the HIGH, and also carries warm equatorial water, forming the Japan Current, north of the HIGH. The greater the intensity of the HIGH the greater would be the tendency to such a change in the distribution of ocean waters; the full effect occurring some months later. Since the HIGH tends to be centered over low-temperature regions, such a change of ocean temperature would result in a southward displacement of the HIGH, thus permitting the storms from the ocean to pass over the coast farther to the south. The result of this would be a relatively heavier precipitation than usual in California.

Gorton (27) offers another physical basis for the relation between summer coastal water temperatures and the following season's rainfall. The coastal upwelling, which is indicated by the temperatures, is a measure of the prevailing northwest wind and hence of the development of the Pacific HIGH, just as McEwen postulates. But, according to Gorton, a relatively great development of the HIGH must be accompanied by subnormal surfaced temperatures throughout the region of the HIGH. These subnormal temperatures will persist for at least 3 or even 6 months. As evidence of the persistence see table 1 reproduced here from Gorton's paper. The resulting subnormal ocean temperatures over a large region during fall and early winter have a pronounced effect on the weather of North America, as shown by Stupart. In particular, the mean position of the polar front in the vicinity of the Pacific coast is brought to lower latitudes. With the normal position of the polar front, southern California is on the fringe of the storm tracks whose maximum passes through Washington. With the more southerly position of the polar front, southern California has more storms and more rainfall than normal. On the other hand abnormally high summer temperatures along the coast are followed by a northerly position of the polar front and almost complete absence of storms in southern California. The reason for using the water temperature for forecasting instead of the pressure in the HIGH, which is more fundamental, is that there are insufficient reports of pressure.

TABLE 1.—*Departures from the normal of surface temperatures in the Gulf of Alaska according to the Kobe reports*

Year	Winter, January–March	Spring	Summer, July–September	Fall
1916				
1917				
1918	−	−	−	−
1919	+	+	+	+
1920	−	−	−	+
1921	+	−	−	+
1922	−	+	−	−
1923	(1)	(1)	(1)	(1)
1924	+	+	+	+
1925	+	+	−	−
1926	+	+	+	+

[1] Data for 1921 not yet published.

The most complete table of rainfall and ocean temperatures that has been published is that given by Gorton in the mimeographed forecast for 1932–33, for the 16 preceding years. (See table 2.) The temperatures are the average of daily (8 a. m.) readings from the 30th to the

41st week of the year. It is seen that for the south coast region during the first 8 years positive departures of temperature are in each case followed by negative departures of rainfall, while negative departures of more than 1.1° were followed by positive departures of rainfall. This
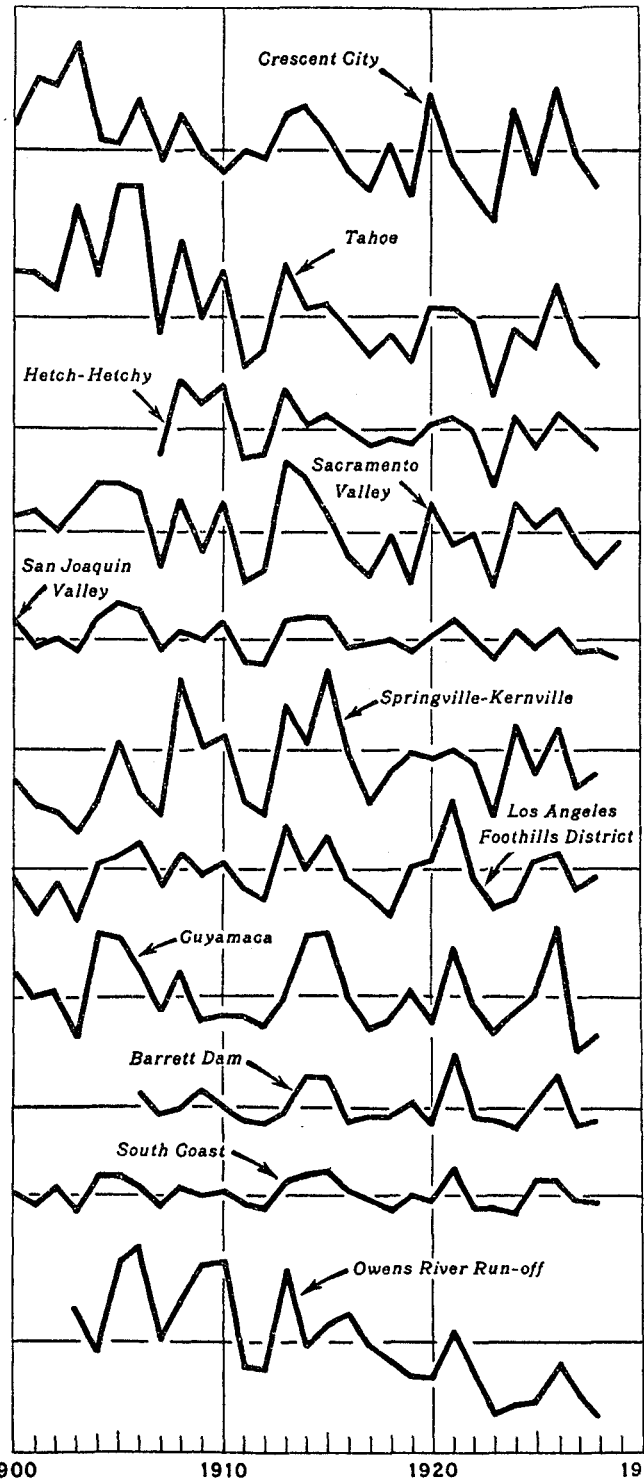

FIGURE 1.—Seasonal precipitation of selected districts in California, 1900–30. Reproduced from Gorton (27).

indicated that the forecasts were valuable for indicating the sign of departure at least. But 1924–25 was the driest of the 16 years, although the temperature in 1924 was as low as any of the 16. Of the remaining 7 years only 3 continue to show the opposite sign.

This repeated failure of the early correlation led to its subsequent use only in conjunction with other indices. Some of these other indices were mentioned in McEwen's forecast of 1928. Though the forecast of 1924 was the worst from the point of view of those using the forecasts, those of 1929 and 1931 were the death blows to this direct use of ocean temperatures, for the physical basis offered

FIGURE 2.—Accumulated departures from normal precipitation and sunspot numbers. Reproduced from Gorton (27).

by McEwen showed that an indication of decidedly deficient rainfall should surely be fulfilled.

This same method was also used in conjunction with water temperatures at Hueneme and Pacific Grove (24) and at San Francisco (32), as well as with air temperatures at San Diego (34), and applied to individual stations (using parabolic equations) in southern coastal California (10) and to other regions in California in various papers, but with no better success.

TABLE 2.—*Average 8 a. m. temperatures and total precipitation from 30th to 41st week of year*

	La Jolla		South coast region		Santa Barbara region (average of 3 stations)	
	Temperature	Departure	Precipitation	Departure	Precipitation	Departure
1916	65.5	−2.1	12.9	0.4	21.42	3.6
1917	68.3	.7	10.9	−1.6	22.05	4.2
1918	69.1	1.5	8.9	−3.6	13.45	−4.3
1919	66.5	−1.1	12.3	−.2	14.25	−3.6
1920	67.4	−.2	10.8	−1.7	14.76	−3.0
1921	66.2	−1.4	21.6	9.1	21.45	3.7
1922	67.8	.2	9.0	−3.5	16.47	−1.3
1923	69.4	1.8	8.7	−3.8	6.35	−11.4
1924	65.5	−2.1	7.6	−4.9	12.45	−5.3
1925	66.9	−.7	16.8	4.3	16.91	−.9
1926	67.4	−.2	17.8	5.3	22.15	4.3
1927	67.3	−.3	11.1	−1.4	14.60	−3.2
1928	66.0	−1.6	10.2	−2.3	12.69	−5.1
1929	69.1	1.5	13.0	.5	12.76	−5.1
1930	68.5	.9	11.9	−.6	13.60	−4.2
1931	70.5	2.9	17.1	4.6	22.00	4.2
16-year average	67.6	----	12.5	----	17.80	----
1932	65.2	−2.4	[1](14.5)	[1](+2)	[1](20.8)	[1](+3)

[1] ( )=indicated.

TABLE 3.—*Correlation coefficients of various indexes with precipitation in northern and southern California*

Index	Precipitation district	Period	Correlation coefficient
Hueneme summer temperature	South coast	1916–29	0.79
Oceanside summer temperature	----do	1921–26	.61
Pacific Grove summer temperature	----do	1919–29	.84
Tokyo September–October temperature	Hetch-Hetchy	1907–28	−.44
Tokyo November–December temperature	----do	1907–28	−.42
Tokyo March–May temperature	South coast	1900–28	−.57
Tokyo March–May temperature	Hetch-Hetchy	1907–28	−.33
Santa Barbara November temperature	Crescent City	1900–27	.86
San Diego September–October rain	----do	1900–27	−.40
Composite index of Hueneme and La Jolla summer temperature	South Coast	1919–29	.84
La Jolla April–May temperature	----do	1916–30	−.47
La Jolla July–September temperature	Hetch-Hetchy	1916–30	−.57
La Jolla July–September temperature	Huntington Lake inflow	1916–29	−.61
La Jolla Upwelling period	----do	1916–30	−.47
La Jolla Upwelling period [1]	Huntington Lake rain	1916–30	−.68
Tokyo September–October temperature [1]	Hetch-Hetchy	1916–30	−.65
Tokyo November–December temperature [1]	----do	1916–30	−.43

[1] The last three correlations were made by successive differences.

In discussing other methods used at Scripps, I will not include the applications of Blochmann's and French's indices (12), or of air temperatures in Japan. (See table 3 reproduced from (27).) Nor will I discuss the simultaneous correlation found between wet and cold seasons. These are of minor importance and were used only temporarily.

It will be noted that less physical basis is offered for the following methods than for the one discussed above. I will first present the two methods which have been used in combination in the latest forecasts of seasonal precipitation, then two methods which were used for a short period, then a method for forecasting air temperatures and one for ocean temperatures.

2. CYCLES

Two periodicities are stated to predominate, the Hellmann cycle (5–6 years) and the Brückner cycle (22–33 years according to Gorton, but actually slightly over 35 years according to Gregory). From a casual inspection of the curves for temperature (33) these periodicities do not appear well marked. The Hellmann cycle, which might be of special use in seasonal forecasting, does not
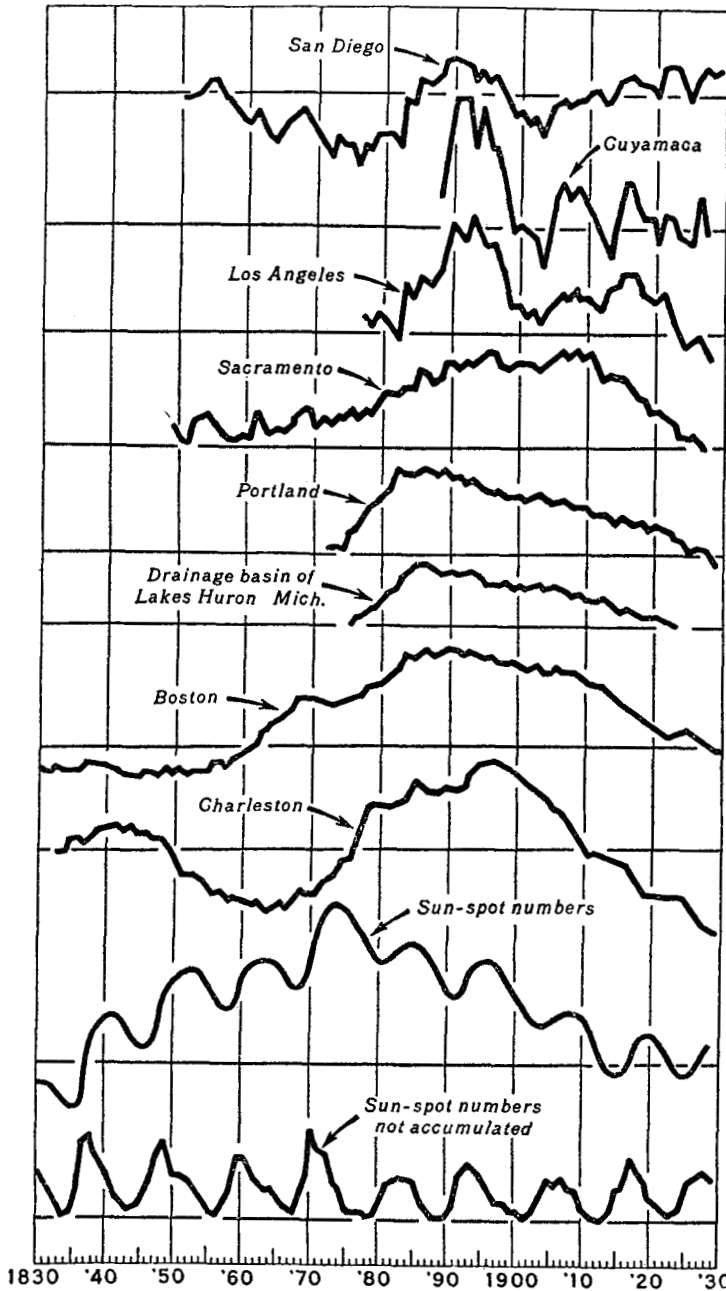
occur at regular intervals. For the air temperature in San Diego (during 1900–31) we have:

Regular intervals		1901	1907	1913	1919	1925	1931
Actual maxima		1900?	1905	1914	1918–19	(1924) 1926	1931
Actual minima			1903	1909	1917	1921	1929

However, there may be cycles whose period varies around 6 years. The precipitation curves (27), here reproduced (figs. 1 and 2), suggest similar periodicities.

The Brückner cycle, which is expected (18) to reach a maximum shortly before 1940, is markedly indicated only in precipitation curve 11 for Owens River run-off and its length can hardly be determined definitely from a 30-year series. In the mimeographed forecast for 1932–33 it was stated that a crest was reached in 1910 and a trough in 1925.

"Cycle factors" are computed for forecasting, in a manner not published, and are used in conjunction with the following method. However, from a casual inspection of the curves, it would not seem that the cycle factors could be reliable for forecasting.

3. DIFFERENCE BETWEEN SUMMER AND WINTER OCEAN TEMPERATURES

In this case the index, X, is the difference between average coastal water temperature at La Jolla or Pacific Grove for the period August 1 to October 15, and the same average temperature for the preceding January, February, and March. The forecasts are for seasonal rainfall in five regions of California and one of Oregon. If Z is the seasonal rainfall and Y the "cycle factor," the formula used in the last few years is—

$$Z = A + BX + CX^2 + DY$$

B is negative for each region except southern coastal California; B and C have opposite signs in each case, and D is always positive. McEwen gives tables (19) showing the observed and computed rainfall for the past 18 years in each of the six regions.

About 75 percent of the signs of the departures of the calculated values agree with those of the observed values. But about 80 percent of the forecasts of a deficiency were correct in sign, and only about 10 percent of the seasons were deficient and not indicated by this forecasting procedure.

Due to the fact that four arbitrary constants were chosen so as to fit the observations (except perhaps the last year or two), the good agreement between computed and observed values does not necessarily indicate great usefulness in forecasting.

Another application of the index X was made by Gorton (34), using the following formula:

$$X' = X - X_{normal}$$
$$Z = A + BX' + CY' + DX'Y'$$

where Y' is the departure from normal of the coastal water temperature for the winter months of the current year. He gives a table of computed and observed rainfall for 16 years and five regions.

The use of Y' is based on his table of correlations between Owens River run-off (in high Sierras) and La Jolla water temperatures for preceding and following months. The highest correlation, −0.52, is found for the current January and February. To determine Y', it must be forecast from the temperatures up to October by the method which will be discussed further down.

Gorton (34) also applied another method which is related to the one above. If x_1, x_2, x_3, x_4 are the departures from normal of monthly ocean temperatures for 4 months during the preceding year, the formula is—

$$Z' = Z - Z_{normal} = ax_1 + bx_2 + cx_3 + dx_4.$$

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The objections to McEwen's method are at least as applicable to these two methods of Gorton's.

4. OPEN OCEAN TEMPERATURES AND SUNSPOTS

A great deal of work has been done by the Scripps Institution in compiling and plotting Weather Bureau ship reports. Their use in forecasting is given in the mimeographed forecast for 1932–33. Particular use was made of the quadrangle from 36° N. to 37° N. and from 125° W. to 127° W. The average sea temperature for January–March was subtracted from that for the following August 1 to October 15. This gives another measure (inverse) of the upwelling and has the advantage that the record covers the 33 years 1899–1931. These yearly differences were divided into low, middle, and high groups, designated a', b', c', respectively.

The average monthly sunspot numbers for the rainfall year (July 1 to June 30) were grouped as follows:

  a equal to or less than 6.8.
  b between 8.8 and 62.1.
  c equal to or greater than 67.3.

In general excess rainfall was found associated with low sunspot numbers.

The expected rainfall was given by the association table—

	a'	b'	c'
a	+	0	0
b	+	+	−
c	0	−	−

"This * * * index gave the correct sign 75 percent to 80 percent of the time over the 33-year period," when applied to various precipitation areas in California.

5. COMPOSITE INDEX

In 1930 McEwen and Gorton state (25) that "the time of occurrence of the maximum ocean temperature (T_m) is within the interval from week number 30 to 41 but varies from year to year. Likewise the amount by which the temperature falls from this maximum value $(T_m - T_{41})$, and the average temperature (11 weeks, T_a) varies from year to year. Strong pressure gradients tend to decrease the time of the maximum estimated from the beginning of the interval, to increase" $T_m - T_{41}$, and to decrease T_a.

It was found necessary to use the departure from normal of T_a, accordingly the index used was—

$$\frac{T_m - T_{41}}{T_a - 62.6°\text{ F.}}$$

which had the same sign as the following seasonal rainfall departure.

Since this index was later abandoned, we may assume that it did not prove very useful.

6. FORECASTING AIR AND OCEAN TEMPERATURES

Gorton (30) attempted to forecast fall and winter air temperatures in coastal southern California on the basis of La Jolla water temperatures during the preceding quarters. Correlations of between 0.55 and 0.66 were obtained. This method was later abandoned in favor of the following one.

Beginning in 1932 forecasts of inshore temperatures have been made regularly 3 months in advance. An

explanation of the method of approach was given by McEwen (19):

While no adequate physical theory for guidance in such problems has been developed, it seems reasonable to assume the existence of factors influencing the temperature trend a few months in advance. While searching for the proper factors, it is assumed that a composite index of them having forecasting value is furnished by past temperatures. Proceeding empirically on this basis, the monthly temperatures are "smoothed" or adjusted in order to eliminate irregularities which at first are regarded as accidental. Projecting these values a few months in advance in accordance with past variations, and correcting the seasonal changes for systematic errors introduced by the smoothing process, provides an empirical forecast. Such a procedure has been applied to observations over a period of about 20 years in order to compare the computed and observed temperatures. A very definite correlation was found, indicating that a temperature forecast 3 months in advance by this method departed from the actual by more than a degree in only 10 percent of the cases for sea temperatures at La Jolla.

In this connection it should be noted that for the years 1916–29 the average magnitude of the monthly deviation from the mean of 13.95° for January was 0.63°, and from the mean of 20.51° for July was 0.60°. (These deviations were computed from a mimeographed table of average monthly surface temperatures at La Jolla.) Of these 28 months only 4, or 14 percent showed deviations of 1° or more. Hence McEwen's verification is not very strict.

Gorton (34) found a close correlation between air temperatures at San Diego and simultaneous La Jolla water temperatures. For the years 1916–32 the coefficients for the 12 months ranged from 0.72 to 0.89, excluding the "transition" months May and November.

Hence, on the basis of the forecast water temperatures, it is easy to forecast the air temperatures (19). During 20 years "temperatures at Riverside were thus forecast to within 2° of the actual in about 90 percent of the cases." However, at Riverside, in the years 1917–36, the average percent of departures from the normal greater than 2° in January, April, July, and October, was only 20 percent, using nearest whole degree in both normal and actual.

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BAUR'S CONTRIBUTION TO LONG-RANGE WEATHER FORECASTING

By I. I. SCHELL

Preface.—In part I of this paper, I attempt to give a critical evaluation of Baur's earlier contributions to long-range weather forecasting which culminated in his experimental seasonal and monthly forecasts. In part II, I describe, mostly in Baur's own words, his method of forecasting for Germany, the 10-day weather trends, during the summer months, and I present some of the principles which underlie this method.

In the preparation of this paper, I am indebted to C. F. Brooks, director of the Blue Hill Observatory of Harvard University and to H. C. Willett of the Massachusetts Institute of Technology for a critical reading of the contents of this paper and valuable personal discussions. I also wish to acknowledge my indebtedness to R. B. Montgomery for kindly sharing with me his critical opinions of several of Baur's investigations.

PART I

Introduction.—Baur's attempt to develop, with the sole aid of statistics, a method for long-range weather forecasting for central Europe, reflects to a large extent similar efforts of a number of other investigators made over a period of many years extending up to the present.[1] The difference between Baur's work and that of the others [2] lies mainly in providing a deeper suggestion of a physical basis for some of the time-lag relationships which were employed. Baur's work is further distinguished by the choice of meteorological elements mainly from nearby regions and by the frequent use of periods immediately preceding the period of forecast. Sunspot variations and secular changes in solar radiation play no direct role in his correlation studies.

[1] Since this report was written, early in 1938, Baur has developed a new method for seasonal forecasting.
[2] Walker's work (reported on elsewhere in this volume) occupies a separate chapter in world weather correlation studies.

1. SOLAR CHANGES VERSUS STATE OF PRECEDING CIRCULATION AS AN INDICATION OF SUBSEQUENT WEATHER

Baur's attempt at long-range weather forecasting does not involve solar variations directly. He is of the opinion (1, 2) that it cannot be proved that there is any connection between changes in the atmospheric circulation and solar phenomena. The basis for the conclusion is, first, that a correlation of the North Atlantic circulation (as given by the pressure difference between Ponta Delgada and Iceland), (*a*) with the contemporary mean monthly sunspot number, or (*b*) with the increase in that number from the past to the current month, or (*c*) from the current to the next month, gave, on the whole, small coefficients. Second: that the large variations in the general circulation of the atmosphere with solar changes are not of the same degree or even of the same sign as would be expected if the change in solar state exerted a marked effect on the circulation. The latter assertion is based on Baur's studies, which, according to him, indicate that the North Atlantic and the North Pacific circulations do not act in unison, and that the subtropical belt of high pressure in the southern hemisphere does not act as a single unit of the general circulation.[3]

Baur contends that there is a close connection, however, between the changes in the atmospheric circulation and the preceding temperature and pressure anomalies over the earth itself. He arrives at this conclusion indirectly by showing that the distribution of pressure in the Northern Hemisphere exercises a systematic influence on the intensity of the North Atlantic circulation, an influence which may either preserve it or change it.

Indeed the above connection appears from maps on which he plotted the average departure of pressure from a 30-year mean (1887–1916) for 44 stations in the Northern Hemisphere, for those months during which the North Atlantic atmospheric circulation was—

(*a*) Very intense and remained unchanged during the following month.

(*b*) Very intense but was succeeded by a weak circulation during the following month.

(*c*) Very weak and remained unchanged during the next month.

(*d*) Very weak but above normal during the following month.

The circulation was regarded as above normal if in the first month for which the distribution of pressure was calculated the departure from the normal monthly gradient between Ponta Delgada and Iceland amounted to at least 4 millimeters (October to April) and 3 millimeters (May to September), and for the succeeding month 2 and 1.5

[3] In a letter commenting on this report, Baur points out that in two papers (58, 59) "striking relatipnships were indicated between sunspots and dry summers as well as cold winters in central Europe, which, however, are not of such a nature that the fluctuations parallel to the sunspots would take place." These relations and an attempt to explain them are also briefly mentioned in the section "Cosmic Influences" of his booklet, Introduction to Broad-Weather Research.
He further points out in the letter mentioned above that, although the effect of changes in solar radiation must be in the same direction in the whole circulation, the fact that the North Atlantic and North Pacific circulations actually differ is a result of the role played by terrestrial laws and is not a proof that solar influence is unimportant.

AVERAGE PRESSURE DEPARTURE

YEARS OF STRONG CIRCULATION UNCHANGED FOR MONTHS INDICATED

FIGURE 1.—Reproduced from Baur (1).

65

AVERAGE PRESSURE DEPARTURE

YEARS OF STRONG CIRCULATION FOLLOWED BY WEAK CIRCULATION

FIGURE 2.—Reproduced from Baur (1).

66

AVERAGE PRESSURE DEPARTURE

YEARS OF WEAK CIRCULATION

AUGUST
1888 1892 1899
1900 1901

SEPTEMBER
1887 1910
1915
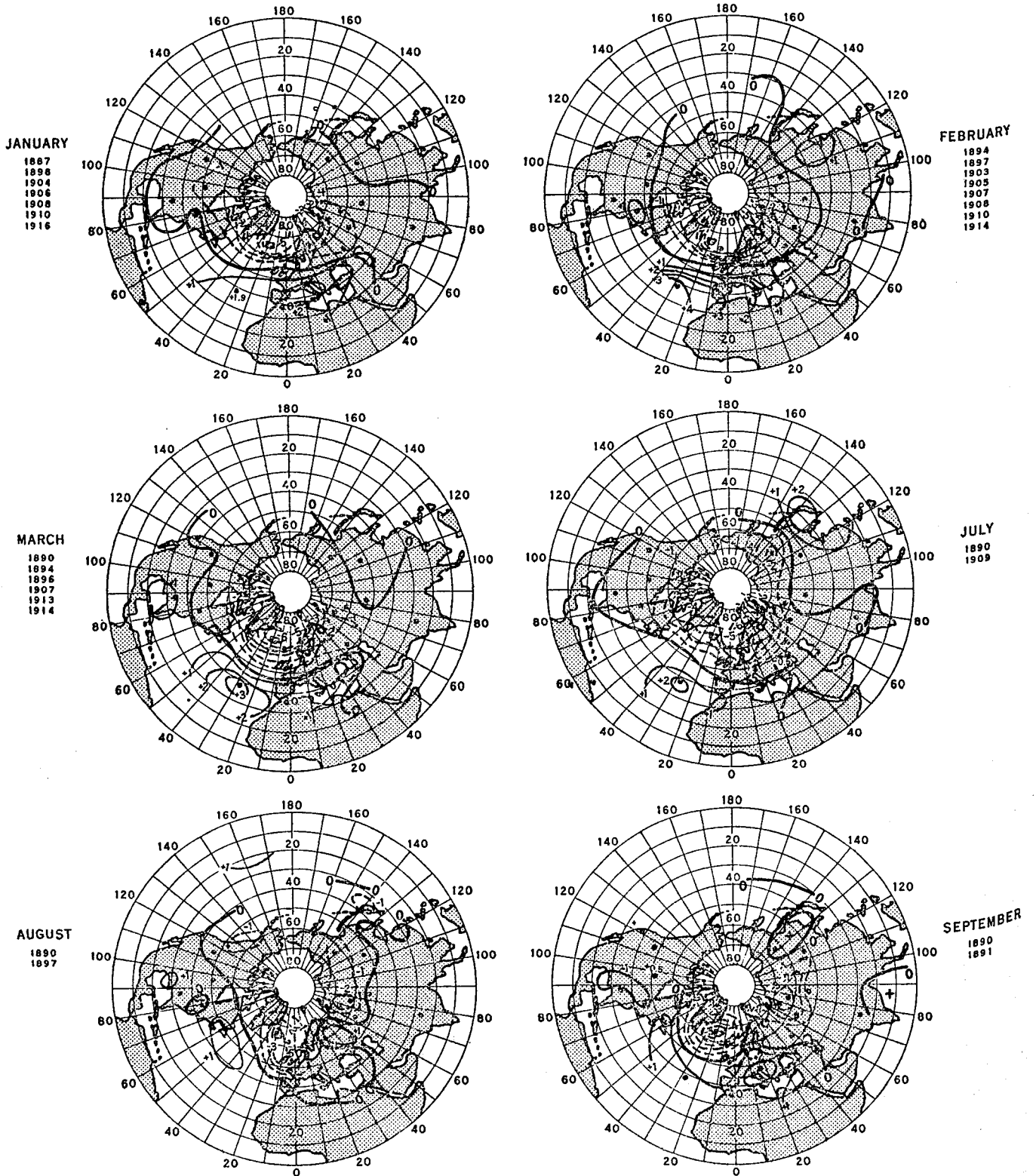
FIGURE 3.—Reproduced from Baur (1).

AVERAGE PRESSURE DEPARTURE

YEARS OF WEAK CIRCULATION FOLLOWED BY STRONG CIRCULATION

FEBRUARY
1890 1895
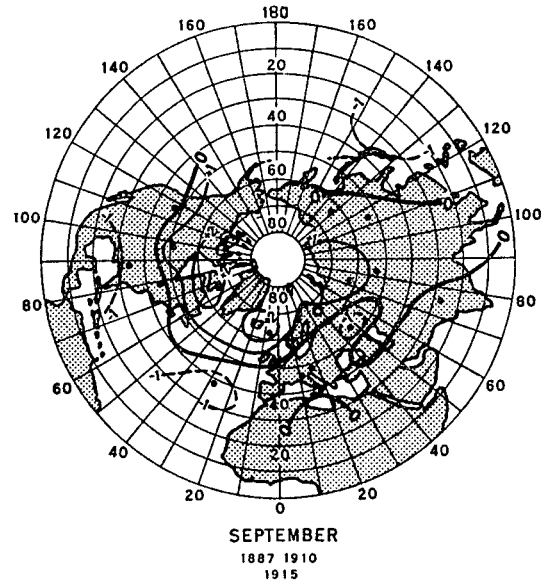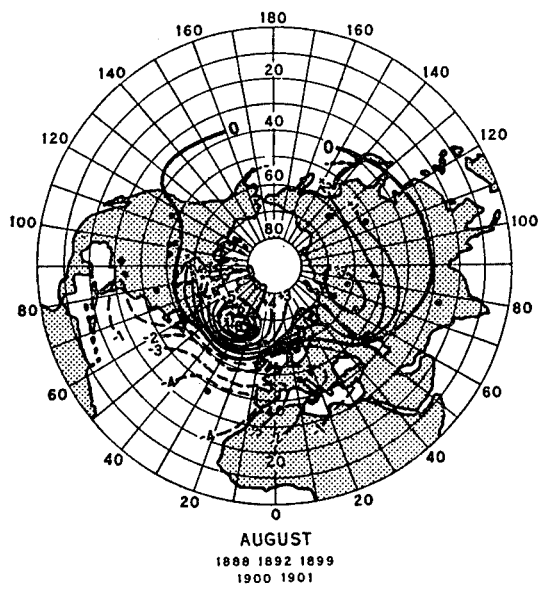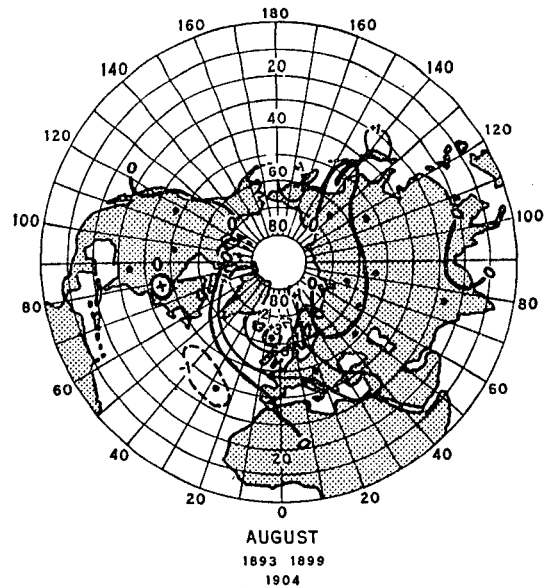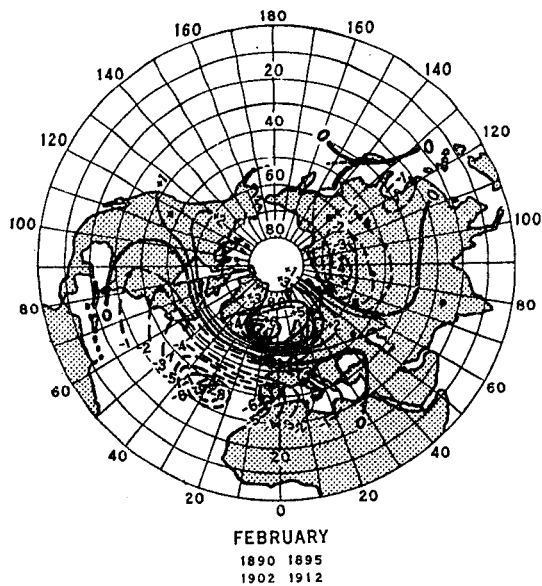1902 1912

AUGUST
1893 1899
1904

FIGURE 4.—Reproduced from Baur (1).

millimeters, respectively, in each case. For illustrations of types (a) and (b) there were chosen 2 winter, 2 summer, and 2 transition months (January and February, July and August, and March and September). For types (c) and (d) February and August were selected. (See figs. 1–4.)

Figure 1 (strong circulation unchanged) shows an approximately circular region of negative pressure departure centered around the pole which extends to middle latitudes. The area of maximum departure is near Iceland. South of that line there is an excess of pressure. Figure 2 (strong circulation followed by weak circulation) shows the region of negative departure in pressure to be confined to narrow, irregular zones, mainly in a meridional direction. Figure 3 (weak circulation unchanged) shows a positive pressure departure over polar and subpolar regions and a negative departure over the middle latitudes. Figure 4 (weak circulation followed by strong) shows irregularly distributed areas of positive and negative pressure departure. It resembles figure 2 except, of course, that the signs are reversed.[4]

Baur concludes thus: that whether an existing abnormality of the North Atlantic air circulation is maintained for some time or is reversed depends essentially on whether the whole polar region shows a regular abnormality of pressure or whether intrazonal contrasts exist in the pressure distribution of high latitudes. Hence the changes in the atmospheric circulation have, for the most part, their origin in the circulation itself, i. e. in the physical properties and dynamic state of the earth's atmosphere, together with the distribution of land and ocean.

It seems to me that Baur's results presented above indicate little more than the apparent fact that the changes in the North Atlantic circulation, as shown by the Azores-Iceland pressure difference, are not directly related to the monthly values of sunspot numbers. Baur's conclusion, on the basis of this and other evidence presented above that solar changes do not exert a dominating influence on the atmospheric circulation may be correct, but the evidence presented by him is far too sketchy and incomplete to enable one to decide the question. His assumption that changes in solar state would be expected to produce everywhere in the atmospheric circulation variations of the same degree or sign is in my opinion unfounded. It should be added that he did not prove whether they do or do not.

Other studies of relationships between solar variations and meteorological phenomena have shown, depending on the time and place, a much closer though extremely varied connection. (See International Research Council, First Report of the Commission appointed to further the study of Solar and Terrestrial Relationships, Paris, 1926, and literature quoted therein.) Rather does it appear from numerous investigations that the present knowledge about relationships between solar and weather phenomena cannot be utilized to account for the irregular changes in the atmospheric circulation nor to develop a satisfactory method of long-range forecasting. Therefore Baur is probably justified, from the practical standpoint in considering directly the state of the preceding circulation for the purpose of eliciting time-lag relationships in the circulation and on this basis to seek a method for long-range forecasting.

[4] Baur verified to a certain extent the relationship between the pressure distribution over the Northern Hemisphere and the Ponta Delgada to Iceland pressure fall by correlating the March pressure at Tromso, Sverdlovsk, Leningrad, Moscow, and Kazan with the Ponta Delgada-Iceland pressure difference in March. Baur also gave several examples illustrating the relationship. He emphasizes, however, that the relationship represents an average condition.

2. THE DERIVATION AND PHYSICAL BASIS OF TIME-LAG AND CONTEMPORARY RELATIONSHIPS

Thus the next phase of Baur's work leading to the ultimate development of his method for long-range forecasts was to find time-lag relationships in the atmospheric circulation. To this end Baur generally used two different and well known methods, one of which involved correlations, the other a synoptic representation of the different trends in the circulation. An attempt to establish a physical basis for some of the relationships suggested by the correlation coefficients, as well as by the synoptic maps, formed an integral part of his work. Since the two methods often were supplementary to each other the relationships indicated will be given here without regard to the method employed in their derivation, but rather insofar as they appear to form a particular system giving rise to relationships some of which were eventually used in forecasting.

Beginning with a consideration of the results shown by figures 1 to 4 (see above) we find, first, that the departures from normal in the North Atlantic circulation are attended by pressure departures elsewhere in the Northern Hemisphere and, second, that the strength of the North Atlantic circulation, under certain conditions, tends to persist from one month to the next, especially at certain times of the year, so that those pressure distributions which correspond to circulations accompanying certain departures will be followed by almost predictable changes in the next month or two. The specific time-lag relationships indicated from the above maps are namely: (1) an above-normal pressure gradient Ponta Delgada to Iceland associated with a negative pressure departure in the circumpolar and subpolar regions is maintained in the following month, whereas (2) an irregular distribution of pressure departure in the above sense is not characterized by a continuance of an abnormally high North Atlantic circulation, while conversely, (3) with a positive pressure departure in the circumpolar and subpolar regions associated with a weak gradient Ponta Delgada to Iceland the gradient tends to remain abnormally low, and finally (4) when an irregularly distributed pressure departure in the polar regions is attended by a weak pressure gradient the persistence of the latter is no longer maintained. Baur, we recall, emphasized that the above results represent average conditions and that only in some instances is the above picture realized. No definite physical basis is suggested for the apparent relationships.

Other time-lag relationships were determined by the correlation method. Two procedures were followed. One was to express first, the state of an element in terms of the contemporary state of other elements in the same region and elsewhere, or in terms of the same element elsewhere, and then to attempt to find similar relationships with a time lag. The apparent advantage of this method is that the physical-synoptic process characterizing the relationships can be identified in a general way and then, assuming that the process involved operates over a longer time than that covered by the original relationship, one can arrive at a physical basis for the ultimate relationship. However, since the latter assumption, on the whole, appears to be invalid the procedure most often followed by Baur was to seek time-lag relationships directly, and, rather than by starting from physical considerations, to attempt to establish a physical basis after the relationships have been derived. In dealing with the correlation coefficients Baur (1) says

that it may be assumed that coefficients equal to or exceeding twice the value of their individual standard errors indicate a causal connection provided that the number of correlation coefficients as defined above is greater than the number obtained from a chance distribution.

By way of illustration of the correlation method of attack employed by Baur, we take the relationship which he derived for forecasting the July rainfall in Germany, as a first step, the conditions defining the character of the July precipitation in western and central North Germany were considered. It was assumed that the rainfall is related to the contemporary pressure in Europe and in the eastern North Atlantic, as well as at Bombay. Upon correlating the monthly values of July rainfall with the monthly values of July pressure at Ponta Delgada,

Figure 7, giving the difference in mbs between the mean pressure in the dry and wet July months, shows the area of maximum difference to be situated over central Europe and thus bears out the fact suggested from the correlations that the closest connection exists between July rainfall and contemporary pressure in the same region. As a physical basis for the relationship Baur declares that the high pressure over central Europe in dry July months is intimately connected with the Azores HIGH and that it can be attributed to processes involving the higher layers of the atmosphere.

The intimate connection with the Azores HIGH presumably shown by the excess of air over Europe in dry July months is, according to Baur, evidently brought about by extended outbreaks of subtropical air in the substratosphere and stratosphere over central Europe.
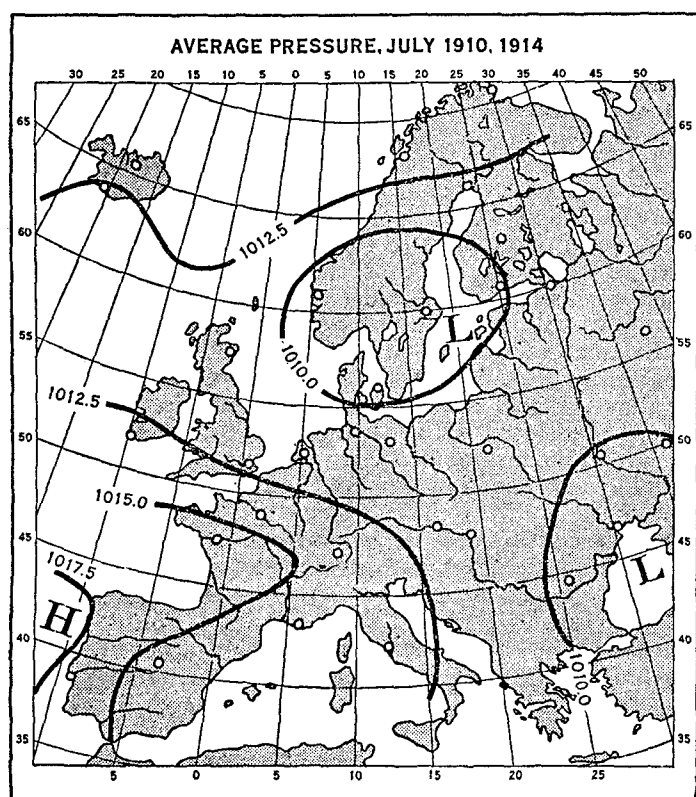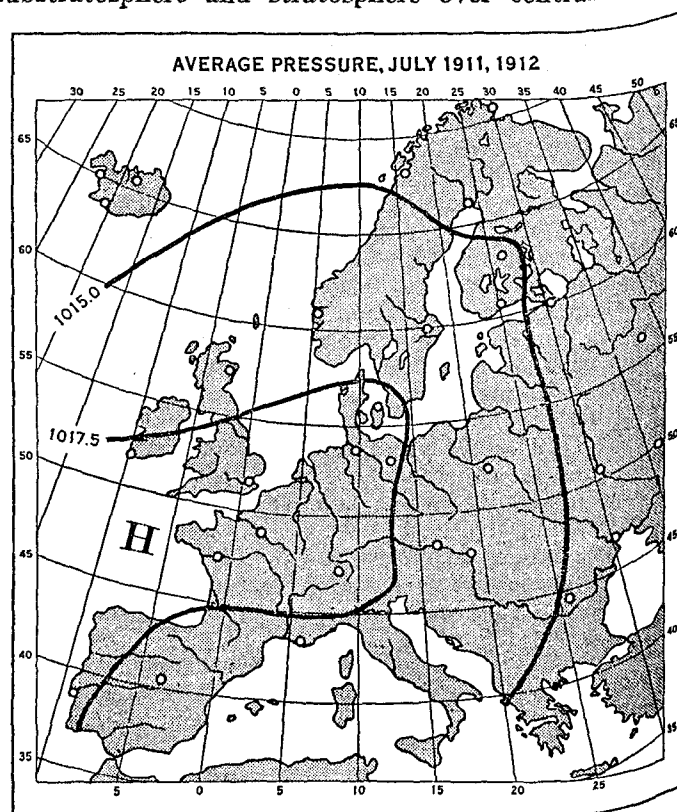

FIGURE 5.—Reproduced from Baur (3).

FIGURE 6.—Reproduced from Baur (3).

Stykkisholm, Jacobshavn, Tromsö, Karlsruhe, Berlin, Höchenschwand, Vienna, and Bombay, Baur obtained a larger proportion of coefficients exceeding twice the value of their standard errors than would be expected, for the central European stations, but negligible coefficients with Ponta Delgada, Stykkisholm, Jacobshavn, Tromsö, and Bombay. The relationship indicated between July rainfall and contemporary pressure in Germany was tested by constructing maps giving the combined mean July pressure distribution separately for 1910 and 1914 and for 1911 and 1912. They were years characterized by excessive and deficient rainfalls respectively in that month. (See figs. 5 and 6.) The chief characteristics of the pressure distribution in the wet July months are low pressure over southern Scandinavia and vicinity, and higher pressure over Iceland than over central Europe. The Azores maximum extends only to the Bay of Biscay.

This type of pressure distribution gives west-northwest winds bringing moist air and rain to Germany. In the dry July months of 1911 and 1912, on the other hand, the Azores maximum extended far over central Europe.

During dry periods the whole subtropical system is displaced northward. In wet months, either these outbreaks take place over the ocean or the subtropical system is farther south. Europe is then dominated by a "polar system." (See fig. 5.) Baur then tries to show on theoretical grounds, that these outbreaks—as shown by the temporary northward displacement of the subtropical high—cannot be caused in any marked degree by an increase in solar heating while the angular momentum of the moving air remains constant. He states that it can be explained only by assuming that the angular momentum of the poleward moving cold air in the stratosphere decreases as a result of mixing with warm air from the north.

Though extensive upper air data are lacking, Baur feels that since the building up of the high pressure area probably occurs gradually, and since marked pressure changes in stratosphere and substratosphere appear also at the surface, one may therefore attempt to find a connection between the July rainfall and the preceding state of the atmosphere as observed at the surface.

Thus Baur's next step was to correlate the June pressure at Jacobshavn, Iceland (2 stations), Tromsö, Ponta Delgada, Bombay, and the June temperature of Iceland, Tromsö, and Germany (10 stations) with the July precipitation in Germany. The period is from 48 to 50 years. Only with the pressure at Tromsö was a correlation exceeding twice the value of its standard error shown. The negligible correlation coefficients involving the other variates can be explained according to Baur, by a change in the weather which ordinarily occurs in the latter part of June. Thus the 5-day pressure maps for June (prepared by E. Alt, Klimatologie von Suddeutschland, Bayer. Met. Jahrbuch, 4, 1919) show according to Baur that on the average the pressure for the last 10 days of that month is indicative of the subsequent July pressure distribution. Also Wiese's result (Met. Zs. 1925, p. 219) shows that the persistence of temperature in central Europe, as indicated from correlations is very small from June to July. For this and other reasons Baur thought it best to investigate the connection between July weather and the pressure distribution of the last 10 days of June, and therefore correlated July rainfall of Germany with the mean morning pressure during the last June decade at a number of stations, mostly in central Europe. A negative correlation is obtained almost throughout. The correlation coefficient [5] for Höchenschwand, high level station, is lower than for neighboring Karlsruhe by 0.02. Other high level stations show a similar trend. This is to be expected, Baur points out, because the stationary, high, warm anticyclone which produces a dry period in Europe, is not as yet fully developed in the last part of June. As additional evidence Baur cites the marked negative coefficients obtained by correlating the pressure difference during the last decade of June between the surface and the 3-kilometer level at Karlsruhe [6] with the July rainfall in Germany. However if the above results and the conclusion drawn from them are valid one would have to imply that conditions in the lower troposphere, during the last decade of June are more significant in the determination of the subsequent pressure than the stratospheric pressure gradient. Baur's reasoning would then have to be modified.

Baur contends that the formation of a stationary anticyclone over central Europe in July requires the existence of a large south-north pressure gradient in the higher levels for some time, and that the existence of the above gradient is evidenced by the appreciable positive coefficients between Tromsö pressure during June 1-20 and July rainfall in Germany.

Is it really reasonable, especially since the correlation coefficients involving Ponta Delgada and Iceland pressures in June and July are negligible, to discuss the question of a south-north gradient on the basis of Tromsö pressure alone? If pressure at Ponta Delgada and Iceland is any indication, the pressure in central Europe in July is not determined by the south-north gradient, though it would appear from the negative correlation of July rainfall with the pressure during the last June decade in Germany that it is influenced by the preceding pressure and in some unknown way by a northward directed gradient. It is unfortunate that neither Ponta Delgada nor Iceland figure in the correlation involving

the pressure of the last decade in June. The positive correlation coefficient with Tromsö pressure during June 1-20 would indicate that abnormally high pressure at Tromsö during that period precedes low pressure in Germany in July and to some extent also in the last decade of June. The latter fact probably explains the negligible coefficient involving Tromsö pressure during June 21-30 and at the same time speaks against the persistence of the south-north gradient suggested by Baur. What probably takes place is that when the polar system is displaced southward it causes a rise in pressure at Tromsö during June 1-20 and a fall to the south (in Germany) during the period, June 21-July 31. The pressure at Tromsö during

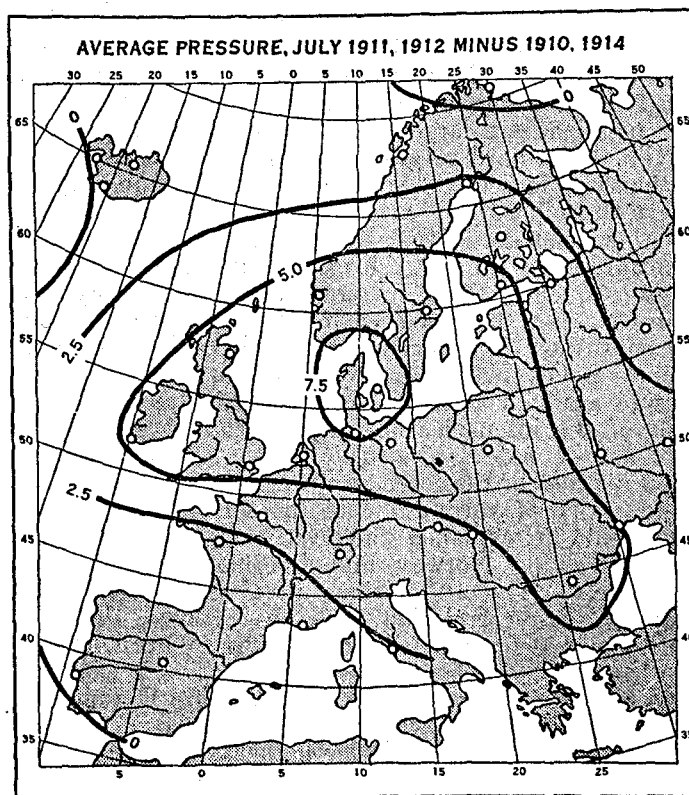

AVERAGE PRESSURE, JULY 1911, 1912 MINUS 1910, 1914

FIGURE 7.—Reproduced from Baur (3).

the latter period is no longer high, relatively speaking, which shows that the original effect was either nullified by conditions it brought about to the south, or that a change in the phase of oscillation took place at Tromsö itself. The apparent change in the last decade of June, which, as we saw above, is significant in foreshadowing events in July, is probably thereby explained.

Thus the evidence presented hardly supports the physical basis suggested by Baur. Baur showed that the pressure at Tromsö during June 1-20 and in Germany during the last decade of that month may be significant in foreshadowing the weather in Germany in July but was able only in part to suggest a reason.[7]

[7] Reference was made to Baur's method of getting upper air pressures. It involved the use of surface observations of pressure and temperature and the assumption of a constant lapse rate (0.55° C. per 100 meters in the case of Tromsö). While Baur himself is aware of the crudity of the above method he nevertheless regards it as good enough for correlation purposes. Yet is this the case? If we assume a moderate variation of 0.1° C. in the lapse rate as used by Baur, we obtain a variation of 1.5° C. in the value of the mean air temperature for a 3-kilometer column. The error introduced in p at the 3-kilometer level is given by—

$$dp = \frac{ghpd\,T_m}{R\,T_m{}^2}$$

where R is a constant, g is the acceleration of gravity, h is the height of the column, and T_m is the mean temperature of the air column. Inserting a value of 273° A for T_m and 1.5° C. for $d\,T_m$, we obtain a value of about 1 millimeter for dp. The correlation coefficient based on true values of p at 3 kilometers might vary considerably from the one where the values of p were calculated by Baur's method. Yet on some occasions Baur goes so far as to draw conclusions from the slight differences in value between certain coefficients.

[5] The various correlation coefficients considered here are rather small. All but one are under 0.3. However each is equal to or greater than twice the value of its standard error. Commenting on their smallness, Baur says that it is due on one hand to the large variety of combinations which may give rise to almost the same decadal means, and that conditions favoring heavy rainfall in July are not quite the opposite of those favoring light rainfall.

[6] The pressure at the 3-kilometer level was computed from surface pressure and temperature by assuming a steady lapse rate of 0.65° C. per 100 meters.

The above example is an illustration of the method used by Baur in deriving clues for forecasting the weather. The main steps involve first a contemporary statistical and synoptic characterization of the element in question; next an attempt to determine the mechanism which operates to produce the existing set of conditions; and finally a study of conditions immediately preceding the given state for clues in foreshadowing the weather.

As another illustration of Baur's studies bearing on long-range forecasting we present the following. The mean monthly pressure difference (50 years) between Ponta Delgada and Iceland was correlated (a) with the contemporary temperature at a number of stations in North America, Greenland, Iceland, and Europe and (b) with the temperature difference between Tromsö and West Greenland. More than half of the coefficients obtained (all but the coefficient involving West Greenland were positive) are larger than twice the value of their individual standard errors. As a physical basis for the relationships indicated by the coefficients Baur suggests, in part, the following:

With an increased circulation a greater flow of warm air northward along the east side of the cell might be expected and consequently a rise in temperature in Norway. To compensate for the increased inflow of warm air an outflow southward must occur. Cold air outbreaks should occur over Greenland because of the stronger west-east pressure gradient between Iceland and Greenland as a result of the intensification of the Icelandic LOW. Such outbreaks should also occur over the Novaya Zemlya region because of the increase in angular momentum due to the mixing of warm and cold air east of the warm current. Thus are explained the positive and negative coefficients involving Norway and Greenland temperatures respectively. The 10 stations in Germany showed a relatively high correlation with the pressure difference Ponta Delgada-Iceland from September to April, December excepted. The high positive correlation is explained by an excess of warm air arriving in western Europe from the ocean when the pressure gradient is large. The lack of correlation in summer and in December is explained by the dominating influence of local heating and cooling respectively. This indication is corroborated by significant positive correlation coefficients between temperature and pressure during June, July, and August and a significant negative coefficient in December.

The high temperatures in North America are explained by an increase in the circulation over the North Atlantic. The break-down of this trend in some months (negative coefficients New York-Halifax) is explained by the influence of the Greenland cold current. At the same time, the existence of high temperatures in North America (Milwaukee) favors an increase in the circulation during the succeeding month.

Baur also plotted the temperature departure (from a 30-year mean) of the Northern Hemisphere separately for years in which the January pressure difference between Ponta Delgada and Iceland was positive and at least 4.0 millimeters and for years when it was negative and of the same magnitude, as well as for July months when the pressure difference was 3.0 millimeters and −3.0 millimeters, respectively. (See fig. 8.) In a measure, the results offer a check of the indications obtained from the correlations.

Baur attempted to introduce a time factor in the above indicated relationships. He correlated the chief variate, monthly pressure difference between Ponta Delgada and Iceland, with the temperature of the following months at Milwaukee, West Greenland (two stations), Tromsö.

The number of coefficients whose value exceeded twice the value of their standard errors was greater than that obtainable by chance. Positive values were obtained for Milwaukee (February, October, November) and Tromsö (January to March, June, August, and November). Baur explains the first by the persistence tendency of the temperature and the latter by the persistence of the Ponta Delgada to Iceland pressure difference.

Baur also correlated the monthly values of pressure at Ponta Delgada with the following month pressure at 21 stations, mainly in the Northern Hemisphere. He concluded from the correlations that the pressure distribution over a large part of the Northern Hemisphere depends on the pressure anomaly during the preceding month at Ponta Delgada. This follows in part from the fact that the cartographic representation of the correlation coefficients shows a systematic distribution (see fig. 9) thus indicating a a systematic connection between the pressure at Ponta Delgada and the following month pressure at the other stations and necessarily a synoptic basis for the relationships in question.

Similarly a correlation of pressure at Ponta Delgada with the second following month pressure at the above stations also shows a larger number of coefficients of apparent value than that obtainable from a chance distribution. In this instance the coefficients for the stations distant from Ponta Delgada are the larger, indicating a space-time propagation of the "effect" of pressure at Ponta Delgada. Baur also points out that the averages of all the coefficients give maximum values in spring and autumn and minimum values in winter and summer, thereby presumably indicating that the pressure anomaly over the Azores has the greatest but not necessarily a direct influence in spring and autumn.

No explanation of the suspected physical basis accompanies either of the above results. While the above investigation falls short of providing an adequate or satisfactory explanation for the relationships elicited therein it established the fact, by no means new, that the state of a particular element at a given point is related in some general way to the preceding state of another element elsewhere, or at the point in question and, in a but little more definite way, to the contemporary state of that other element.

As a final illustration of Baur's method of eliciting time-lag relationships, his attempt (4) to arrive at an expression for forecasting the March weather character of Germany is given. Because of the different and more comprehensive nature of the problem the supplementary steps which he employed differ somewhat from those used in his other studies. Similarly the physical basis and reasoning advanced by Baur are novel and suggestive.

As in earlier attempts Baur's first step was to define the March weather character in Germany in some suitable fashion. It appears that between the March pressure and the contemporary rainfall there is a high negative correlation but none with the temperature. Baur therefore chose to classify the weather into four types:

A. High pressure, warm, dry, predominantly clear, daily temperature range above normal.
B. High pressure, cold, dry, predominantly clear, daily temperature range above normal.
C. Low pressure, warm, rainy, predominantly cloudy, subnormal daily temperature range.
D. Low pressure, cold, rainy, predominantly cloudy, subnormal daily temperature range.

He represents each type by the contemporary average devia-

AVERAGE TEMPERATURE DEPARTURE

YEARS OF LARGE POSITIVE AND NEGATIVE PRESSURE GRADIENT
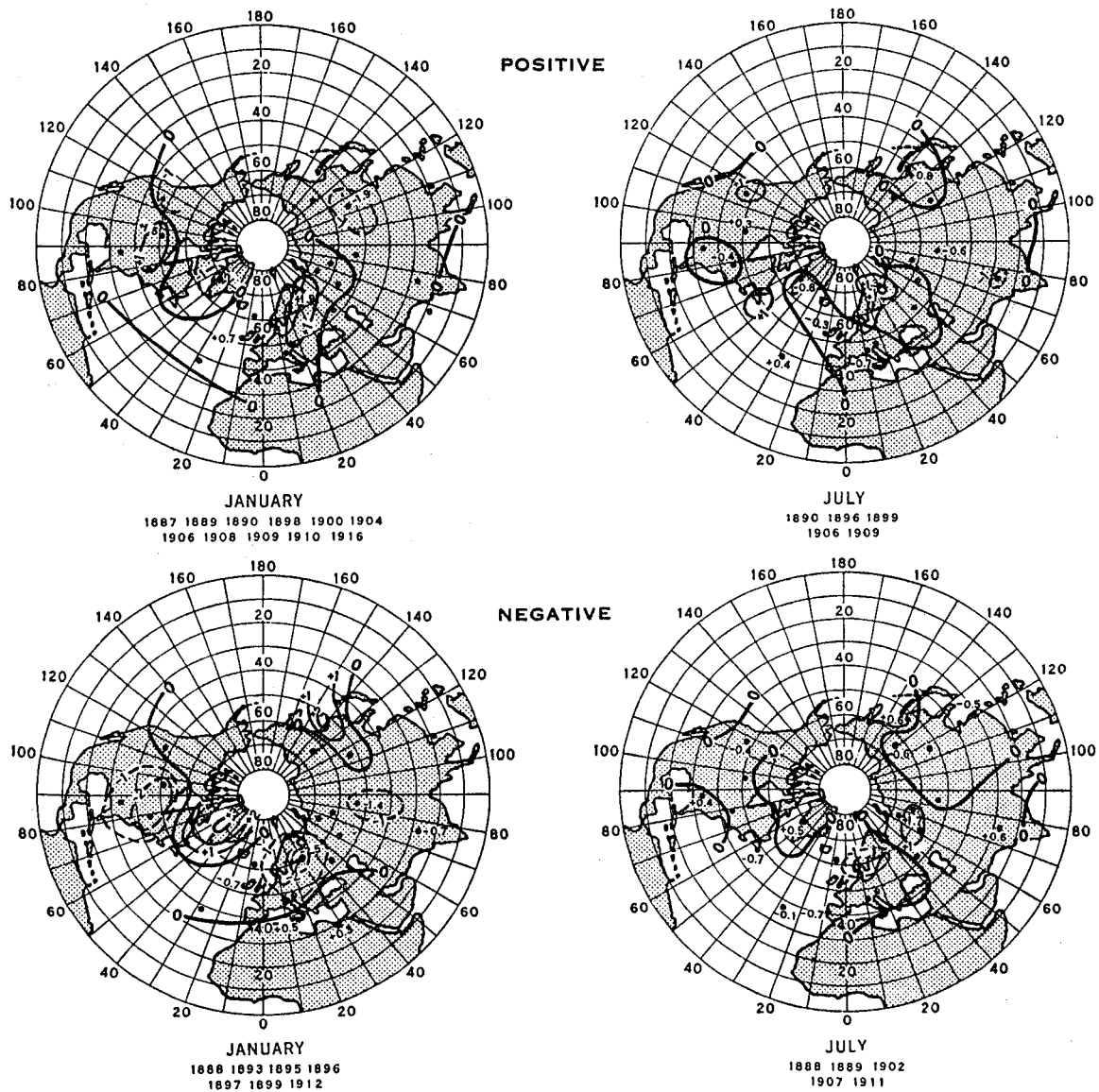DEPARTURE, PONTA DELGADA, AND ICELAND

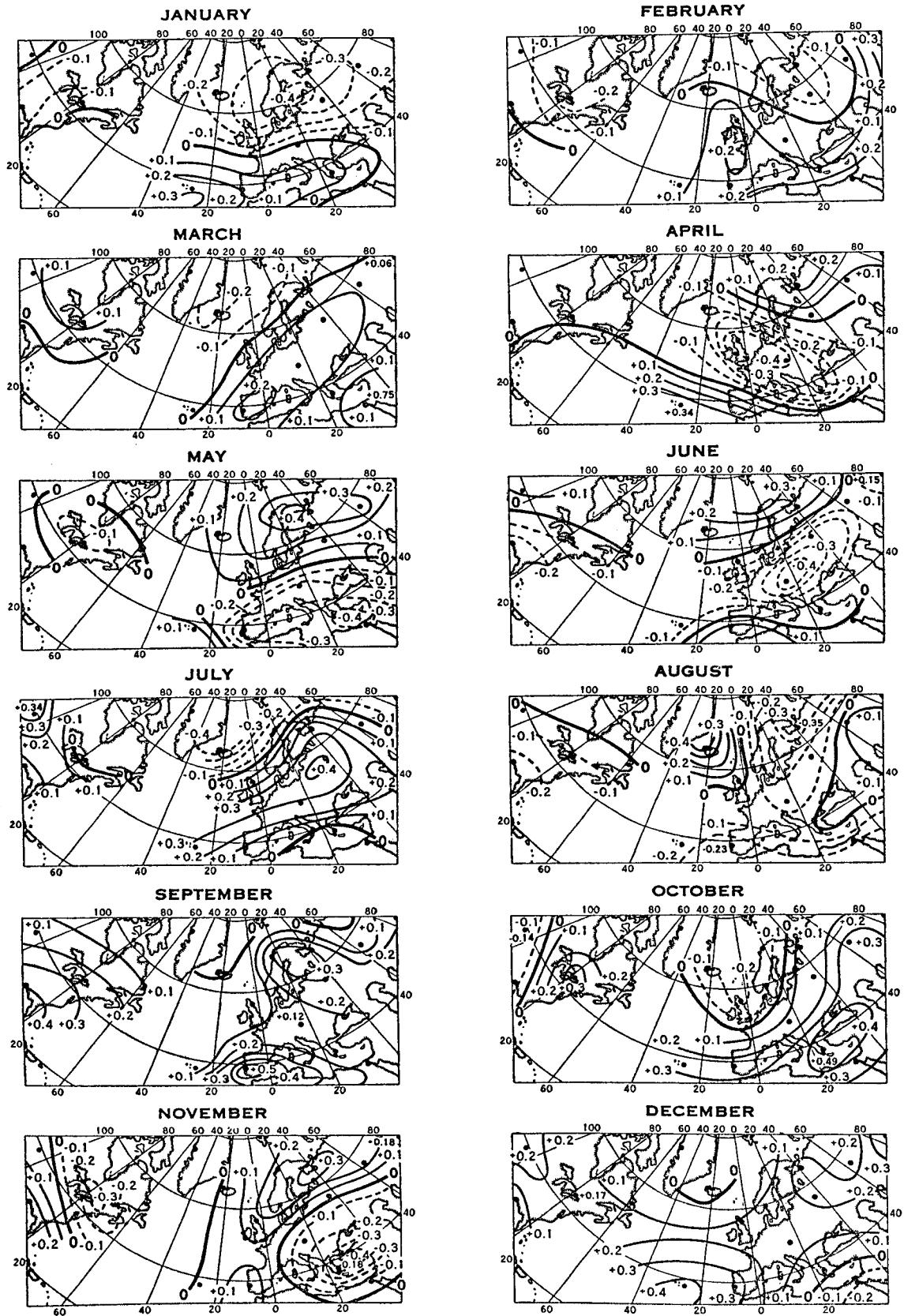FIGURE 8.—Reproduced from Baur (1).

FIGURE 9.—Correlation coefficients between pressure in month indicated and Ponta Delgada pressure in previous month, 1874–1903. Reproduced from Baur (1).

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AVERAGE PRESSURE DEPARTURE, MARCH,
W I T H
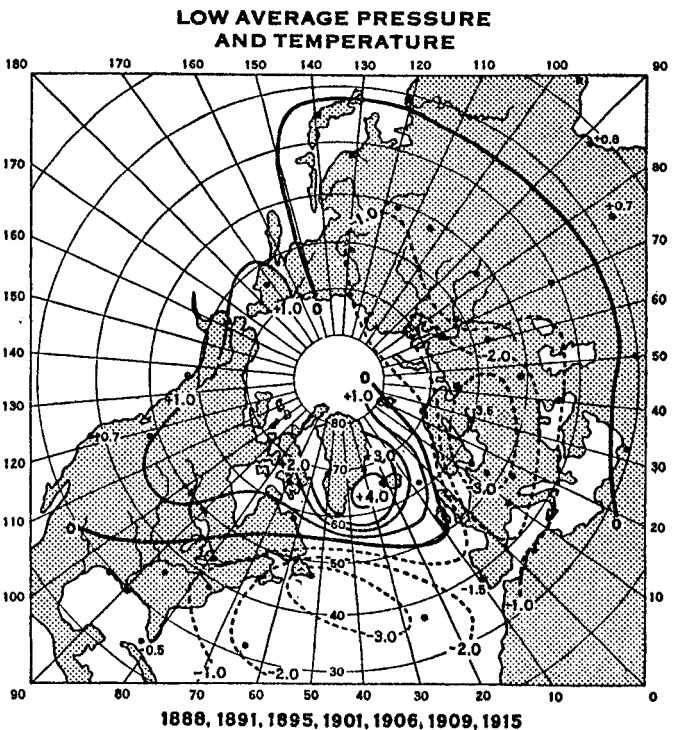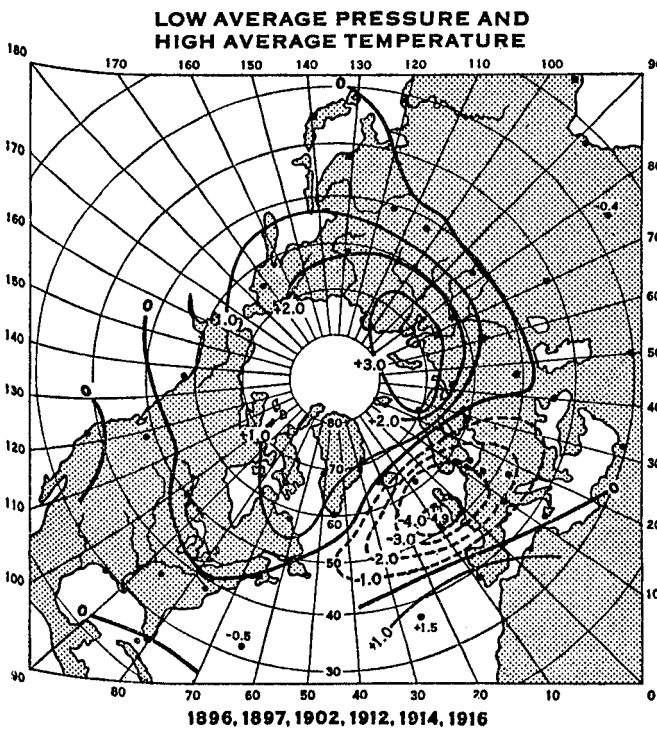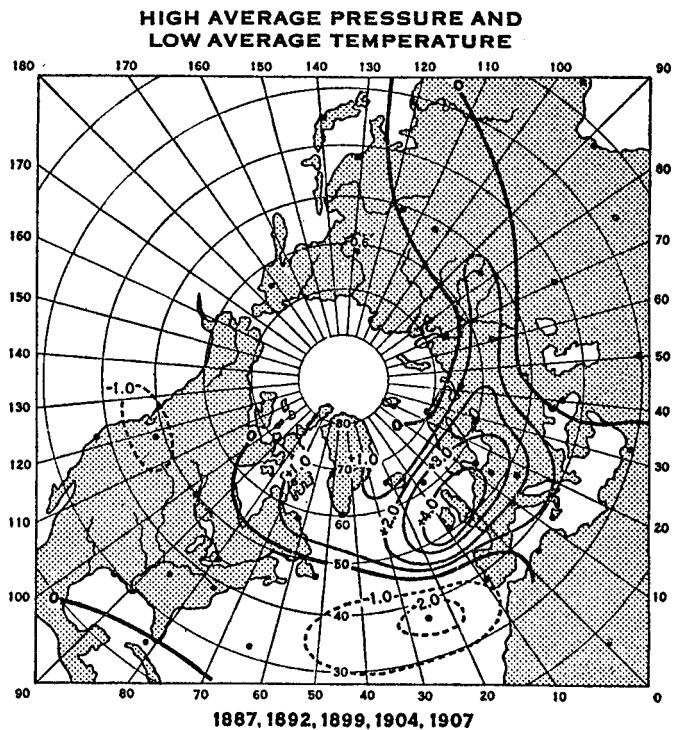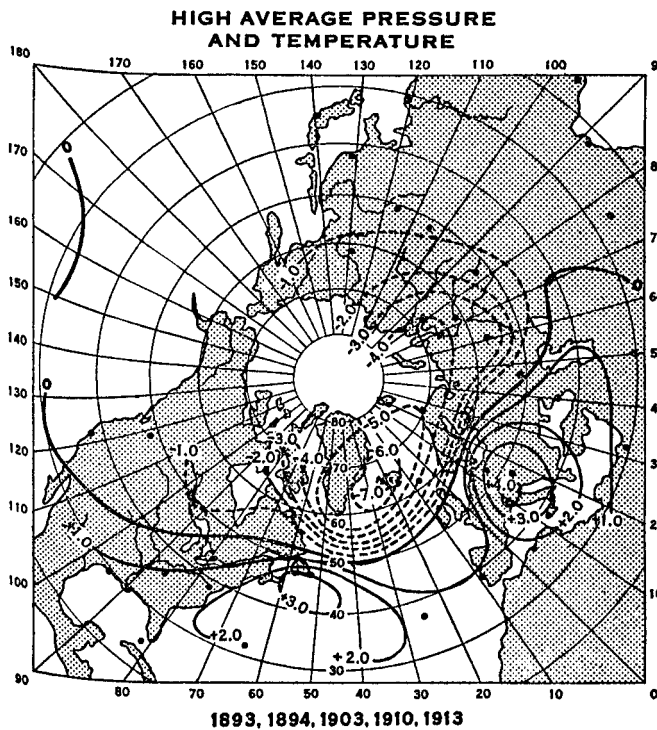
HIGH AVERAGE PRESSURE
AND TEMPERATURE

1893, 1894, 1903, 1910, 1913

HIGH AVERAGE PRESSURE AND
LOW AVERAGE TEMPERATURE

1887, 1892, 1899, 1904, 1907

LOW AVERAGE PRESSURE AND
HIGH AVERAGE TEMPERATURE

1896, 1897, 1902, 1912, 1914, 1916

LOW AVERAGE PRESSURE
AND TEMPERATURE

1888, 1891, 1895, 1901, 1906, 1909, 1915

FIGURE 10.—Reproduced from Baur (4).

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AVERAGE TEMPERATURE DEPARTURE. MARCH,
WITH

HIGH AVERAGE PRESSURE
AND TEMPERATURE

1893, 1894, 1903, 1910, 1913

HIGH AVERAGE PRESSURE AND
LOW AVERAGE TEMPERATURE

1887, 1892, 1899, 1904, 1907

LOW AVERAGE PRESSURE AND
HIGH AVERAGE TEMPERATURE

1896, 1897, 1902, 1912, 1914, 1916

LOW AVERAGE PRESSURE
AND TEMPERATURE

1888, 1891, 1895, 1901, 1906, 1909, 1915
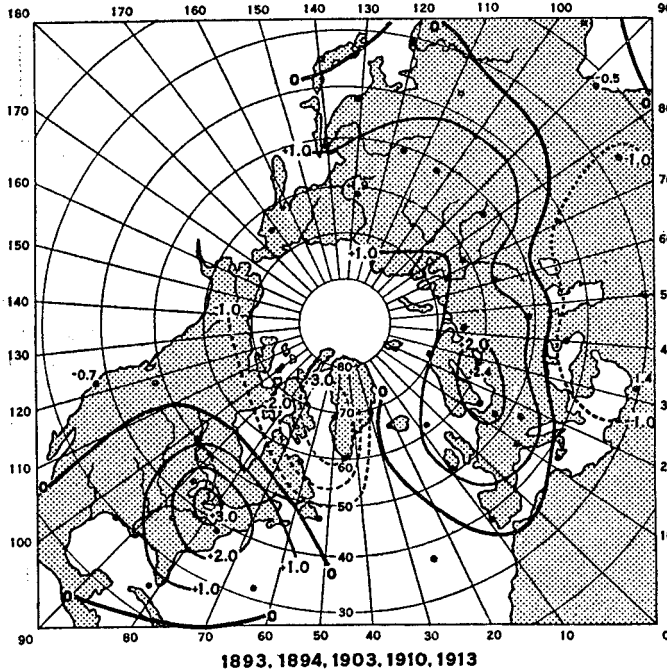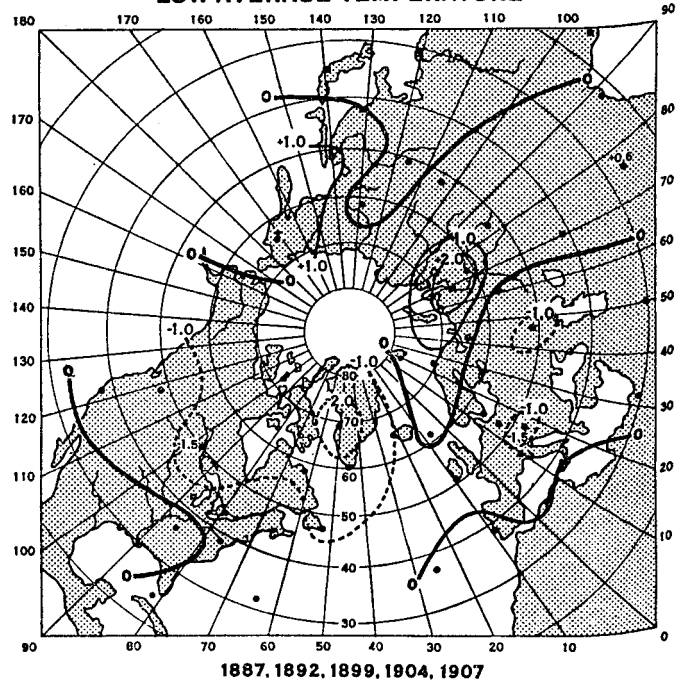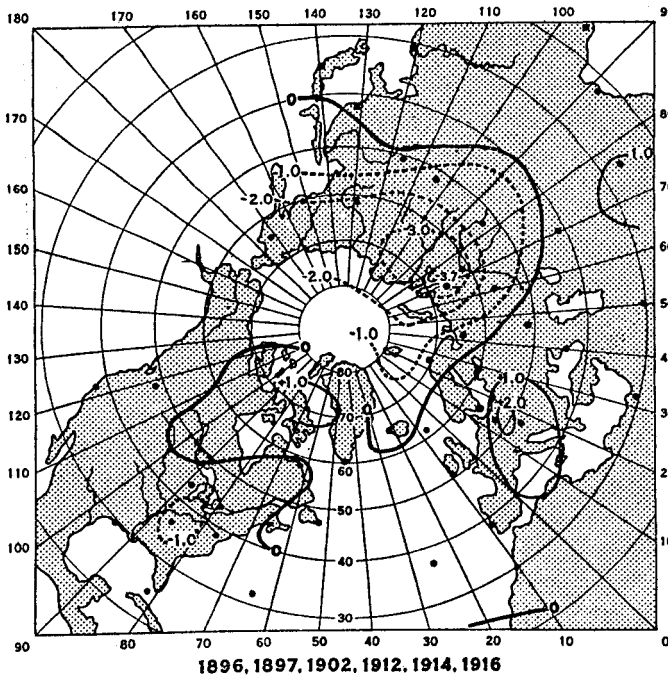
FIGURE 11.—Reproduced from Baur (4).

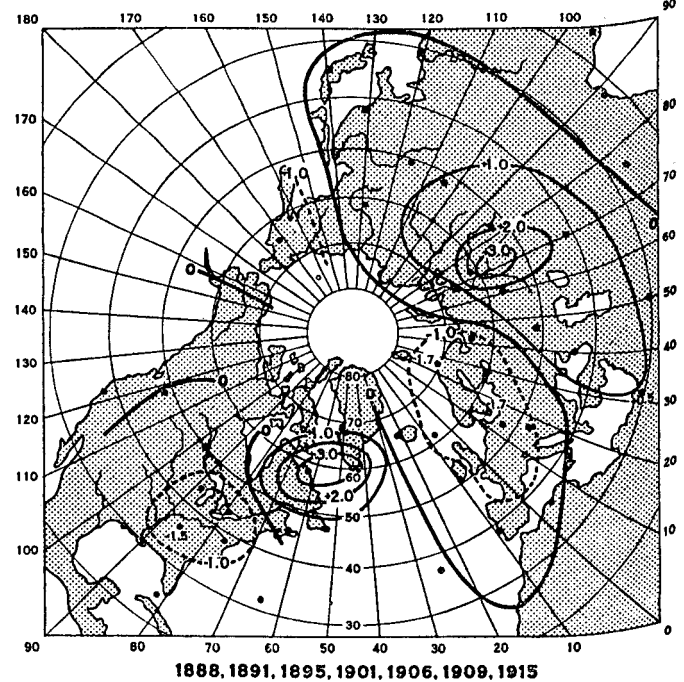tions from a 30-year mean of pressure and temperature at 49 stations over the Northern Hemisphere. (See figs. 10, 11.) It is thereby noted that the above four types occur with nearly the same frequency. The criteria for assigning a type to each March were two: (1) A deviation (from the 1874–1923 mean of 755.7 millimeters) of at least 1.5 millimeters in pressure at Berlin; and (2) a deviation (from the 1870–1923 mean) of at least 0.3° C. in the mean temperature at 10 stations in Germany.

Another method of representing some phases of the March weather character is by means of the contemporary south-north pressure gradient, Ponta Delgada to Iceland (mean of Stykkisholm and Berufjord) and the east-west pressure gradient, Irkutsk to Iceland. The coefficient between the former gradient and the temperature in Germany (1874–1922) is +0.71, and between the temperature and the latter gradient (1879–1924) +0.65. Since these two pressure gradients are closely allied (correlation=+0.85, 1879–84, 1886–1924), the question arises whether a warm March in Germany is due to the southerly or westerly winds, or both. An actual investigation of the wind components during March showed that in general the southerly component is more important for the temperature than the westerly. Baur finds this reasonable since the continental-maritime temperature difference is small in spring. A somewhat detailed consideration of the individual types shows that with weather in Germany of types A and D the pressure difference between Ponta Delgada and Iceland is important but with type B or C the pressure fall from Lugano (south foot of Alps) to Greenwich is important. Thus for the 26 months of March during 1874–1923, when pressure and temperature departures in Germany were of the same sign (type A or D), the correlation of temperature in Germany with pressure difference between Ponta Delgada and Iceland is 0.85; for the 24 months of the opposite sign (type B or C) the correlation with pressure fall Lugano to Greenwich is +0.71.

Having broadly defined the March weather character in Germany in terms of the contemporary state of some other elements, Baur introduces a time-lag in some of the relationships treated above. He correlated the March temperature with the pressure at various stations in the preceding February. With the exception of Kem, Irkutsk, and Tokio, these are for the period 1875–1924. The marked features of these two charts are an area of positive and negative correlation respectively over the Mediterranean and over northern Europe. Figure 12 shows that the March temperature is probably indicated to a certain extent from the pressure distribution in Italy and northern Europe. A strengthened zonal circulation in February, characterized by an excess of westerly winds, is apparently followed in March by a weakening of this circulation as indicated by the large correlation with the Irkutsk-Iceland pressure gradient. The weakened zonal circulation is followed by another giving predominantly southerly winds which is determined by the Irkutsk-Iceland gradient.

Baur explains the apparent relation of the strengthened zonal circulation in February to the March temperature as follows: Since apparently the strengthening is not equally intense all around the earth in this zone, accelerations and decelerations arise:

Wegen der Erhaltung der Kontinuität der Massen muss dann mit der Zeit in den Gebieten mit verzögerter Bewegung eine Luftmassenanhaufung, in Gebieten mit vermehrter Bewegung eine Massenverminderung stattfinden, sofern nicht durch meridionale Bewegungen ein Ausgleich geschaffen wird, was auf der rotierenden Erde das Vorhandensein zonaler Druckunterschiede ausreichender Grosse zur Voraussetzung hat.

It is therefore conceivable, Baur says, that an increased transport of air over Europe in a west-east direction, in February, which is brought about by a positive pressure anomaly over Italy and negative anomaly over northern Europe is conducive to an abnormal pressure gradient from inner Asia to Iceland in March, and vice versa.

In support of his argument Baur gives a correlation of 0.46 between the south-north pressure difference ½ (Lugano+Rome)−½ (Tromsö+Haparanda) in February and the east-west pressure difference Irkutsk-Iceland in March (1879–1924). At the same time the persistence correlation of the Ponta Delgada to Iceland pressure fall (February to March)is only 0.24 (1874–1923). Thus Baur concludes that the persistence of temperature in Germany from February to March (+0.53, 1874–1923) is not due to a persistence of the general character of the

CORRELATION OF MARCH TEMPERATURE IN GERMANY WITH PRECEDING FEBRUARY PRESSURE AT STATIONS SHOWN, 1874-1923
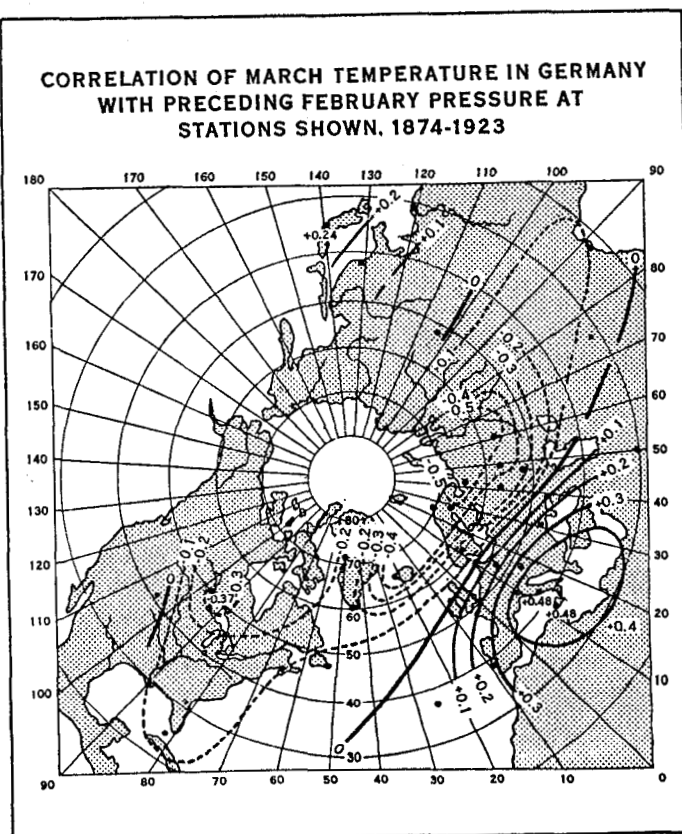
FIGURE 12.—Reproduced from Baur (4).

circulation in that region, but that general conditions in February giving high temperature in Germany tend to produce a definite but different set of conditions in March which also give high temperature in Germany.

Baur makes no attempt to prove the validity of his basic assumption, namely, conservation of continuity of flow under the conditions postulated above. Indeed it would appear that they are violated in a large measure. The isocorrelation lines (see fig. 12) do not indicate much conformity with Baur's hypothesis of convergence or divergence of flow. This criticism however need not be construed as a rejection of the hypothesis. The limited evidence precludes further discussion of this point.

The above three illustrations can be taken as representative of Baur's methods of attack and his results. They indicate that the various methods lend themselves to eliciting very limited relationships between the state of a particular element at a given place and the contemporary as well as

noncontemporary state of other elements elsewhere or at the same place, but that the results obtained by such methods often suggest a physical-synoptic basis.

8. MATHEMATICAL FORMULATION OF MULTIPLE RELATIONSHIPS AND THEIR APPLICATION

Baur's next step in the utilization of the derived time-lag relationships was to form expressions which would enable one to compute the value of a given element in terms of several others to which it is related. As a preliminary step to their application, however, he considers whether the available observations lend themselves to use in the usual correlation method. They do, he states, provided (1) the relationship is linear, (2) the number of observations is large, and (3) the distribution of observations under comparison approximates the Gaussian. Baur regards the above conditions fulfilled, the first on the assumption that relationships which involve weather anomalies of distant regions and different time-lags, are linear; the second, if the available observations extend over a 50-year period or so; the third, if it satisfies the actual test.[8] Baur admits that the relationships may not be strictly linear, but assumes that the errors due to non-linearity in a multiple regression equation will not be important.

As an illustration of the application of Baur's method we can take his experimental attempt at forecasting the seasonal temperature in Germany (5). He first regards the temperature in Germany as being a function of the circulation over the North Atlantic Ocean (given by the Ponta Delgada to Iceland pressure fall), also of the intensity of the Asian anticyclone in winter, of the pressure at Bombay which is a measure of the state of Asian LOW in summer, of the pressure in Argentina, which was found to be closely related to the subsequent weather in the Northern Hemisphere; of the temperature in the eastern half of the United States by virute of the general west-east flow; of the preceding temperature in Germany because of persistence and finally, for general reasons, of the temperature in northern Norway and western Greenland. By way of support of his thesis that an actual physical basis underlies the relationships, Baur shows that of the 282 correlation coefficients involving the above variates 23 exceed twice the value of their standard errors as compared to 13 that might be expected from a random distribution.

Baur correlated all of the above variates, except the Siberian anticyclone for which no data were available, with the temperature for each of the four seasons. Then with the aid of the statistically derived approximations he computed the sought-after element for each year of the period on which the approximations were based as well as for several seasons in advance.

The approximation for the winter temperature, for example, is—

$$y \sim 0.253x_1 + 0.233x_2 - 0.326x_3 - 0.717x_4 + 0.128x_5$$

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[8] An example of the test of the distributions of observations for their approximation to the Gaussian distribution is given below:

Temperature:
West Greenland, August–October 1874 to 1923 $\psi(x) = \psi(x) - 0.00665\psi_2(x) - 0.0022\psi_4(x)$
Germany, spring 1870 to 1923 $\psi(x) = \psi(x) - 0.0137\psi_3(x) - 0.0028\ \psi_4(x)$
Germany, summer 1870 to 1923 $\psi(x) = \psi(x) - 0.0035\psi_3(x) - 0.0040\psi_4(x)$
Germany, autumn 1870 to 1923 $\psi(x) = \psi(x) + 0.0037\psi_3(x) - 0.0081\psi_4(x)$
Germany, winter 1870–71 to 1923–24 $\psi(x) = \psi(x) + 0.0240\psi_3(x) - 0.0062\psi_4(x)$
Pressure difference:
Ponta Delgada-Iceland, spring 1874–1922 $\psi(x) = \psi(x) + 0.0104\psi_3(x) - 0.0087\psi_4(x)$

The negative coefficients of $\psi_4(x)$ of the distributions reproduced here show that the large departures are more numerous, and that the smaller departures occur less frequently than demanded by Gauss's law. The coefficients of the term $\psi_3(x)$ indicate that the distributions about the mean are not symmetrical, the plus sign denoting that the positive departures predominate while the negative values are larger, and vice versa. However, all the coefficients are sufficiently small so that the correlation method in its usual form may be applied.

where—

x_1 = temperature, Germany, preceding February to June.

x_2 = temperature, Alten and Vardo, June to October.

x_3 = temperature, West Greenland, July to August.

x_4 = pressure difference, Ponta Delgada to Iceland, April to June.

x_5 = temperature, eastern United States, preceding September to November.

The standard error of estimate of the above expression is 1.451° C., whereas the standard deviation of the 50-year mean winter temperature is 1.632° C. The mean error introduced in the computation of the winter temperature is therefore 13 percent less than the deviation of the winter temperature itself.

Baur sets up limits of ±1.5 times the standard deviation above or below which the season is considered "very warm" or "very cold," respectively, and ±⅓ the standard deviation, within which the season is defined as "almost normal."

The comparison of the computed and the observed departures of temperature made by Baur showed that in 29 out of 196 cases the departures were outside of the expected limits. This, Baur points out, is two cases less than the most probable number from a chance distribution. The expressions derived were tested in forecasting for the following 5 seasons not included in the determination of the approximations. The agreement between the forecast and observed temperature departures was equally satisfactory on the basis defined above. As an example, I reproduce here the forecast of temperature for the summer of 1925.

The probability is 84 percent that the departure of the mean summer temperature will be between 0.69° C. and −1.24° C.; 98 percent that the summer will not be very warm; 89 percent neither very warm nor cold. The actual departure was 0.1° C. as against −0.28° for the computed departure. Forecast considered completely realized.

As another illustration of the application of relationships we take the computation of monthly values of July rainfall. The correlation coefficients of north Germany rainfall (west of the Oder) in July (y) with Karlsruhe 7 a. m. pressure June 20–30 (x_1), and with Tromsö 8 a. m. pressure June 1–20 (x_2), and between Karlsruhe and Tromsö pressures (x_1, x_2) lent themselves to the following approximation for the departure from the mean of July rainfall in North Germany:

$$y \sim -2.51x_1 - 3.13x_2$$

The standard error of estimate = 20.10 millimeters. This is 8.4 percent less than the standard deviation of rainfall for the period 1875–1924. Considering as very dry or very wet months whose rainfall departure is greater than one-third of their normal monthly values, the statistical probability of occurrence of one of the extremes over the period 1875–1924, is thirteen-fiftieths, or 26 percent.

It appears that the cases of nonagreement of sign between the computed and observed rainfall values are distributed equally over the entire period. From this Baur concludes that a relationship similar to the above could have been derived earlier and hence the obtained expression may be assumed to have been discovered in 1874. A comparison of the computed and actual departures for the 21 years when the former was more than 8.4 millimeters shows that the "forecast" departures agreed with the actual as to sign in all years except one. Baur points out that in some years one could have made

more significant forecasts. If the July rainfall be considered as normal so long as the departure is not greater than 8 millimeters (one-tenth of the normal amount) and if by too dry or too wet is understood simply the sign of the departure, then the following forecasts could have been obtained:

1877: July with an 86 percent probability of normal or too dry.
1893: July with an 87 percent probability of normal or too wet.
1898: July with an 81½ percent probability of too wet.
1899: July with an 83 percent probability of too wet.
1900: July with an 89 percent probability of normal or too wet.
1917: July with an 87 percent probability of normal or too wet.
1921: July with an 88 percent probability of normal or too dry.
1923: July with an {88 percent probability of too dry. 78 percent probability of a negative departure greater than 8.0 mm.

These eight forecasts would have all been correct.

4. DISCUSSION

Baur's method of forecasting seasonal and monthly values of meteorological elements is based on the consideration that the statistically derived relationships lend themselves to expressing the probable occurrence of the approximate value of an element, and that the relationships, as indicated by statistical or by synoptic criteria, have a physical basis. In the case of the seasonal forecasts, the existence of a physical basis is only implied. Thus Baur does not explain why or how the winter temperature in Germany is determined by the preceding February to June temperature in that region, by the pressure in Argentina during the preceding April to June, etc.; or why the spring temperature correlates negatively with the preceding October to November temperature in Germany. The question of the soundness of the method reduces itself then to the question of the validity of the statistical considerations. It is taken up below.

In case of the monthly forecasts a physical basis for the relationships employed is very often suggested. However we saw from the preceding sections of this paper that the basic assumptions which Baur made regarding the atmospheric circulation appear to be either doubtful or wrong. Even if we allow the assumptions it does not appear that Baur was able to provide a definite and adequate physical interpretation of the various relationships. It was not shown for example that the upper south-north gradient over Europe during June determines the July rainfall in Germany, or that the March temperature in that region is governed by pulsations of the west-east flow of air in February. This leaves the question of a physical basis for the underlying relationships unanswered. The physical basis advanced by Baur cannot be accepted yet nor can it be definitely rejected. The question of the soundness of Baur's method for forecasting monthly values reduces itself then mainly to the validity of the statistical considerations.

In the application of the regression equations to forecasting two fundamental assumptions are tacitly made, one that the time-lag relationships involving weather anomalies of distant parts of the globe are linear, and the other, that these relationships are stable with respect to time. With regard to the first assumption it is generally believed that on the whole such relationships may be treated as linear.

With regard to the second assumption, i. e., whether a given relationship will be maintained in the future, or has held in the past, it may be noted that the stability of a coefficient expressing meteorological relationships is in-

fluenced by a certain periodicity or rhythm of weather phenomena but perhaps mainly by marked deviations of the atmospheric circulation from the average state. To quote E. I. Tichomirov's discussion (J. Geophys. and Meteor. vol. IV, No. 1, 1929, Leningrad [9]) on the effect of "periodicity" on the stability of coefficients:

We know that a certain rhythm, if not periodicity, exists in the weather * * * Egersdorfer's investigations (Das Jahrbuch von Bayern, 1925) showed that when phenomena having a periodic nature are correlated, the time interval within which the correlation occurs will have a large influence on the magnitude and even the sign of the coefficient * * * Supposing for example that we take 50 years. If we acknowledge the reality of Brückner's rhythm, then it is obvious that the correlation coefficient will vary with the position of the 35-year interval within the 50-year period. Since in most cases the position of the correlation interval in relation to the rhythm of the phenomenon is not, a priori, known, the question of the reality of the apparent connection and stability of coefficient appears doubtful from the point of view of forecasting with the aid of the correlation method. Therefore the investigation of the stability of the correlation coefficient should always precede its use in forecasting.

However this cannot be done rigidly, i. e., by employing rules of statistical mathematics, since, because of a rhythm in meteorological phenomena, one cannot speak of independence of consecutive values in a meteorological series. At the same time the hypothesis regarding their independence forms the basis of criteria employed in statistics. One must therefore resort to a rougher check of the stability by dividing, for example, the time interval in question in sufficiently large parts and then compare the results obtained. In practice, one can of course speak of a division into two groups, 20-25 years each. Baur proposes that a connection be regarded as stable if the differences between the coefficients of the two divisions does not exceed the sum of standard errors of these coefficients. A check on the stability may also be made by forming a series of "moving" correlation coefficients. The behaviour of such a series may give us a picture of their stability. This can be seen from the following example. Schmauss computed moving correlation coefficients for separate 11-year periods of the period 1879-1924. The coefficient for the whole series was −0.32. The results of the computations are given below. The year at the top is the middle year of each interval.

	0	1	2	3	4	5	6	7	8	9
1880					−56	−57	−44	−47	−46	−51
1890	−63	−64	−61	−38	−24	−25	−39	−41	−36	−20
1900	−14	−10	3	16	−18	−33	−33	−20	−13	−27
1910	−34	−26	−36	−54	−47	−38	−38	−39	−45	−33

From this Tichomirov comes to the conclusion that an extrapolation during years nearest to the end of the period considered is sometimes quite lawful. Baur comes to the same conclusion but from different considerations. He says (5) that—

since some of the relationships are probably due to periodic variations and since in course of a long period the amplitude and phase of a shorter period changes several times it is quite possible that at least a portion of the relationships existing during an earlier period are later replaced by others. Nevertheless since the relationships are endowed with a physical basis the assumption may be allowed that the relationships which dominate the weather changes over a 50-year period would also be maintained in the next 3 to 5 years.

In contending that the stability of the correlation coefficients remains sensibly constant for the next few years the effect of nonperiodic changes in the atmospheric cir-

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[9] Osnovnye pryemy predskazanii pogody na dolgi srok (Principal methods of long-range weather forecasting).

culation is neglected. But this effect is often very important.[10]

In the last analysis the merit of Baur's method of forecasting seasonal and monthly values of meteorological elements lies in the accuracy of the forecasts and their usefulness. As far as it is known to me no independent verification of his seasonal and monthly forecasts was ever undertaken. Regarding their usefulness Tichomirov's discussion (loc. cit.) is appropriate.

Baur gives the probability with which we can expect that the value of the element in question will be included within a certain interval. In practice it is desired that the interval be as small and probability as large as possible. Both of these requirements are met only when the ratio m/s (m., standard error of estimate; s, standard deviation) is very small, but in the regression equations published up till now it is not less than 65 percent, a value which should not be exceeded. Whether we like it or not it is thus necessary, if the results are to have any significance, either to decrease the probability or lengthen the interval.

The limited usefulness of Baur's forecasts (6, 7, 8) is further exemplified by the following statement which is a part of one of his forecasts. "Therefore one must figure on at least one severe cold outbreak in spring temperature in Germany in 1926. Whether it will occur in March or in April can not be said at the present."

5. SUMMARY AND CONCLUSIONS

Baur's work in long-range weather forecasting consists of a derivation, with the aid of statistical methods, of a number of time-lag relationships involving, on one hand, monthly or seasonal values of meteorological elements in central Europe and on the other hand similar values representing the preceding state of these elements in the same and distant regions. With the aid of these relationships the future approximate state of certain meteorological elements could be determined with a certain degree of probability.

Baur's attempt to establish a definite physical basis for the statistically derived relationships proved fruitless; nevertheless, he was able to provide a suggestion of the physical-synoptic basis in several instances, namely, where the relationships involved relatively small time lags, and where the choice of variates was confined to the same and neighboring regions.

It is necessary to point out that the statistical approach in long-range weather forecasting involves a serious limitation; namely, there is no assurance that the relationships will hold after the period for which they were derived. Most of the relationships derived up till now from correlation studies, and employed in forecasting did not hold afterwards. The reason for this is the periodic and nonperiodic changes in the atmospheric circulation. The disturbing influence of the periodic factor can be reduced to a certain extent by recomputing the correlation coefficient so as to include the last observation. Unfortunately, the nonperiodic factor which at times is very important cannot be determined because the nature of the circulation is not properly understood. It is worth while to note at this point that the correlation studies show that such an understanding will be difficult to attain.[11]

Some time-lag relationships do exhibit a marked stability (9). Unfortunately there is no way of telling which

of the relationships will remain stable and which will not. Yet it is reasonable to assume that a forecast based on a number of relationships which have been stable for a long time will probably verify to a marked extent; for it is unlikely that all the relationships will break down at once. There is therefore hope in forecasting with the aid of linear regression equations, provided caution and intelligence are exercised in deriving the proper relationships, as was done in British India, for example.[12]

The use of the above approach entails, however, another severe handicap which considerably limits its usefulness at least for certain regions and seasons This is the nature of the results that are possible of attainment. For regions where the fluctuations in the weather within seasons are great, western Europe and the United States, for example, a forecast of normal temperature for the fall or spring is of limited value; in fact, it may be at times of considerable harm. On the other hand, for countries like India whose weather is dominated by land and sea monsoons such a forecast would be of decided worth; similarly for other countries where the weather fluctuations are small. Yet, it should be noted that even for the United States, especially for certain parts of it, the weather fluctuations are relatively small during the summer and therefore a forecast for this season might be of value. It follows, that in applying the statistical approach the first step is a consideration of the meteorology of the region for which the forecast is to be made.

Baur's experience was helpful in showing that with the methods used by him and described above, no seasonal forecasts of decided value could be obtained for Germany thus far.

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Supplementary bibliography

[The following bibliography represents those of Baur's works which have or may have direct bearing on the problem of long-range weather forecasting and which were not directly referred to in this paper.]

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[10] Wiese, upon correlation of the mean Leningrad temperature of 1 month with that of the following obtained (Met. Zs. 1925) for one 30-year interval r_v, $_{vi}$=0.56 and r_{vi},$_{vii}$=0.47 yet for the following similar interval, 0.02 and −0.10 respectively. How much of the change in the coefficients is due to the periodic and how much to the nonperiodic effects cannot be said.

[11] The confusion regarding the proper place of statistics in long-range weather studies is very great and is best attested by the results achieved so far. Meteorological laws do not appear to be sufficiently complex to be governed by laws of chance; yet neither are they simple enough to be explained by the existing mathematical physical theories.

[12] In a letter commenting on this report, Baur maintains the opinion gained through his research results of the years 1925–28, that with the help of linear regression equations alone no sufficiently reliable monthly and seasonal weather forecasts can be made. He comments further: "The principle of the free multiple correlation index (used as the basis of the 10-day forecasts) into which the relations enter in their original form—without being pressed into an analytical function determined beforehand—has also proved efficient for the monthly and annual forecasts; only the number of empirically given cases is much smaller for greater periods than for shorter. In any case, it is always necessary that all influences be considered for a long-range weather forecast. At the present time a monthly or annual forecast can be made if in a certain case all methods and recognitions lead to the same result. The experience has taught that in such manifold insured cases the forecasts prove correct. On the other hand, viewing the present status of the research, a regular issuing of monthly and seasonal weather forecasts would not be advisable."

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limited application, because, in most cases, it is impossible to obtain from a monthly mean a good characterization of the weather trend. It is of little value to know, for example, that the month's temperature is going to be normal if the first two weeks are below and the second two above that normal. At the same time a consideration of individual weather situations represented by daily weather maps appeared to offer an inadequate basis for long-range weather forecasts. This led Baur to introduce, in practice, a meteorological concept which was originally evolved by Teisserenc de Bort, in 1881, but which,

AVERAGE PRESSURE DISTRIBUTION,
FEBRUARY 5-20, 1929

FIGURE 13.—Reproduced from Baur.

because of technical and other reasons, found no application in long-range weather forecasting at that time. The concept which serves as the central idea in the theoretical and practical foundation of his method received the name "Grosswetterlage." Sir Gilbert Walker's translation of the above expression "broad-weather situation" is adopted here.[13]

1. BROAD-WEATHER SITUATION

A proper understanding of the above term merits the inclusion of Baur's own discussion and Walker's amplification of this term. Baur (5) distinguishes between "Wetter" (weather), "Witterung" (weather trend), and "Grosswetter" (broad-weather or general type of the weather). By weather is meant the instantaneous state of meteorological elements at a given instant or, also, the outstanding characteristics of the successive states of the elements during several hours or a day. The "weather trend" gives the characteristic total of the weather of individual days over a period of several days. As a rule the two concepts refer to a particular place or region, where the same weather or weather trend prevails. The concept "broad weather," in common with "weather trend," represents the atmospheric occurrences during several days or even during several weeks and months. However, it is more comprehensive in that it takes in not only a particular region but also the neighboring regions, provided there is a direct physical connection between the individual meteorological processes of all the regions. Thus, with the same broad-weather situation given by a certain form of the general pressure distribution the weather trend of west Germany may be different from that of east Germany. The term "Grosswetter" is further defined by Baur (6) as a macro-perturbation—

a notable departure from the normal state of the atmosphere which maintains the same sign, experiencing at the most very slight breaks, over large areas of the earth's surface during at least 3 days and under certain circumstances during many weeks.

To this may be added Walker's explanation (3): "it (the broad-weather situation) is a condition of the atmosphere which controls the weather for several days, remaining sensibly constant in spite of the changes in the latter from day to day."

The employment of the principle of "broad-weather situation" necessitates the consideration of time intervals whose length is determined by the prevalence of a particular weather type or broad-weather situation. These intervals, experience showed, were on the whole, much less than one month for central Europe. As an example I reproduce from Baur's work the broad weather situation covering the period February 5–20, 1929. (See fig. 13.) During the 16-day period the mean temperature at Potsdam was −12.8° C., Treuburg −16.9° C. The period was also very dry. The high pressure area over Europe is in direct "contact" with the Siberian anticyclone. The pressure gradient over central Europe is from north to south. This is associated with a cold flow of air from the east.

The final choice of 10 days as a convenient time interval to represent the most frequent broad-weather situation was not arbitrary, Baur claims, but was determined by the frequent presence of a 5-day rhythm in central European weather.[14] One of the first tasks which confronted Baur was to form a systematic, comprehensive collection of pressure maps which determine the particular weather types or broad-weather situations of central Europe. These maps were accompanied by a detailed delineation of the associated weather developments. The material is represented by several hundred maps.

The determination of the coming broad-weather situa-

[13] Baur was preceded in the adoption of this concept in long-range weather forecasting practice by the Russian school of long-range forecasting, who define the broad-weather situation in terms of fields of pressure system centers and anticyclonic trajectories instead of the actual pressure distribution, as done by Baur.

[14] The choice of 10-day periods was determined in part by the annual variation in insolation and by the existence of so-called singularities in the annual course of all meteorological elements.

tion was made in part from a consideration of the distribution of pressure aloft. The acceptance of this principle is based on results of several recent investigations and considerable experience which indicate that the main weather development in the middle latitudes appears to be determined by the pressure distribution and its variation aloft and not by the surface pressure distribution.

2. ROLE OF THE STRATOSPHERIC PRESSURE DISTRIBUTION IN DETERMINATION OF THE BROAD-WEATHER SITUATION

The consideration that the stratospheric pressure gradient normally south-north in the Northern Hemisphere and the general air flow, normally west-east in middle latitudes materially influence the formation and movement of areas of high and low pressure led Stüve, Mügge, Baur, and others to investigate the effects of a deviation from the normal meridional temperature and pressure gradients on the life history of the above pressure systems. Stüve showed (7, 8, 9) how the pressure changes at the surface may vary when, with a normal south-north pressure gradient in the stratosphere, the temperature gradient in the troposphere is north-south instead of south-north, as normally. Baur found that the pressure gradient in the stratosphere, at least in its lower levels, is not always directed south-north. The deviation from the normal direction, sometimes observed, comes about according to Baur through a northward displacement of the equatorial high-pressure area in the stratosphere, or even through the formation of nuclei of high pressure from the southerly cell (10). As an example, Baur cites the high pressure over Kiruna, Sweden, about 68° N., during the period August 19 to 24, 1909. The pressure between 12 and 15 kilometers was as high level for level, as it is normally at 30° latitude. That we are actually dealing with a high-pressure area follows, according to Baur, from the fact that similarly high pressures were observed at Kiruna on preceding days, that the height of the tropopause was around 13 kilometers (considerably above the normal for that region); that the temperature in the lower stratosphere was below normal while in the troposphere it was above; and that the interdiurnal variability of pressure over northern Sweden was very small. To investigate the relation between the meridional pressure gradient aloft and the general flow of the atmosphere, Baur considered, first theoretically, how far down a moderate gradient at 10 kilometers, say 1 mb in 111 kilometers will extend if a temperature gradient of 1° in 111 kilometers in the layers below has the same direction as the pressure gradient. For a pressure of 266 mb at 10 kilometers, in latitude 50°, in summer, and a temperature of 247° A. as the normal mean temperature of the air column between 4 and 10 kilometers the pressure gradient becomes zero at 3.2 kilometers above the ground. Thus, with only a moderate pressure gradient in the substratosphere opposed by a considerable temperature gradient below the flow of air in the greater portion of the troposphere is determined by the pressure distribution aloft. Baur concludes that the general flow of air is therefore most intimately connected with the pressure gradient in the lower stratosphere.[15] As empirical evidence Baur offers the observed movement of areas of 24-hour pressure

[15] From a comparison of the absolute and relative topography of the 500 mb surface (relative to the 1,000 mb surface) Baur found that out of 458 high-pressure areas at 5 kilometers only 22 were characterized by other than warm air in the layer below. Correspondingly 428 out of 452 LOWS were distinguished by relatively cold air below. The few exceptions, Baur remarks, are obviously phenomena of transition since they come only on individual days. From the above, Baur concludes that, in general, the temperature gradient in the troposphere has the same direction as the pressure gradient in the lower stratosphere to which it is also similar in strength.

changes[16] at the surface and especially at 5 kilometers as related to the pressure distribution at 5 kilometers. The movement of the pressure change fields takes place in the same direction as the general flow, and hence perpendicular to the upper pressure gradient. The above relation was termed steering by the Frankfurt school.

Two types of steering are distinguished, simple and double steering. Several examples of steering are repro-
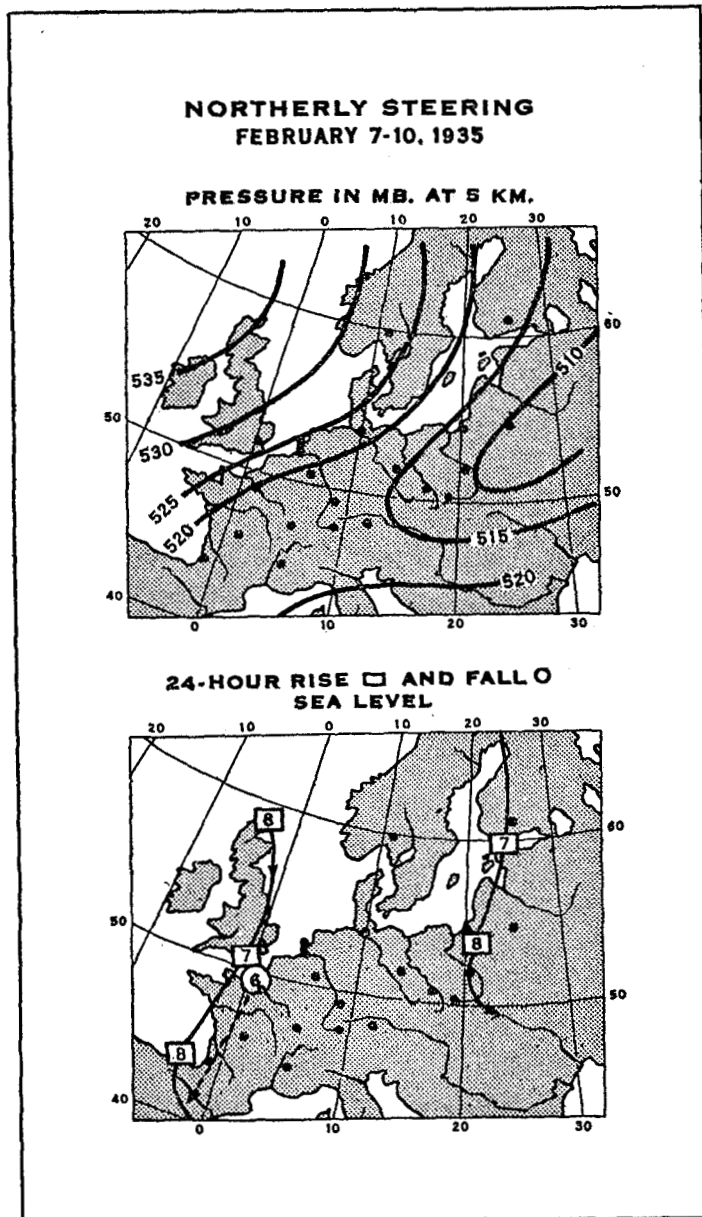
FIGURE 14.—Reproduced from Baur (10).

duced here from Baur's work (loc. cit.). Figure 14 shows the tracks of pressure change areas during February 7 to 10, 1935, to be almost parallel to the direction of isobars at the 5 kilometer level, even to the extent of the turn eastward. Figure 16, giving the mean height of the 500 mb surface (over the 1,000 mb) shows the lines of equal potential difference to follow almost completely the isobaric lines, and hence shows the association of a high-pressure area aloft with a warm air mass below it, and

[16] The changes of pressure in 3 hours are less under control because they depend on the motion of the warm and cool air masses of the lower troposphere, so that the changes do not persist for 24 hours.

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conversely. The tracks are predominantly from north to south and this is a case of northerly steering. Easterly steering is represented by figure 15. The pressure gradient is north-south, the reverse of the ordinary. Though the temperature gradient is also reversed (fig. 16), the relation does not break down. The general flow is almost from east to west. The winds at 8 kilometers (fig. 19), indicate the existence of the abnormal gradient at higher levels as well. It is interesting to note, as Baur points out, that at this time no fronts were shown over central Europe and

sphere is very weak, the pressure distribution at 5 kilometers fails to indicate the nature of the pressure gradient in the stratosphere. Then the winds in the stratosphere and upper troposphere may be essentially different from those below. We deal here with two flows of air. The control governing these flows is called double steering. Figure 18 shows a ridge of high pressure to the north. However the movement of the 24-hour isallobaric areas is neither from southwest to northeast or from northwest to southeast but from west to east, both at sea level, and even more so at 5 kilometers. The last is in agreement with the westerly winds at 8 and 10 kilometers and indicates a south-north pressure gradient in the lower stratosphere,
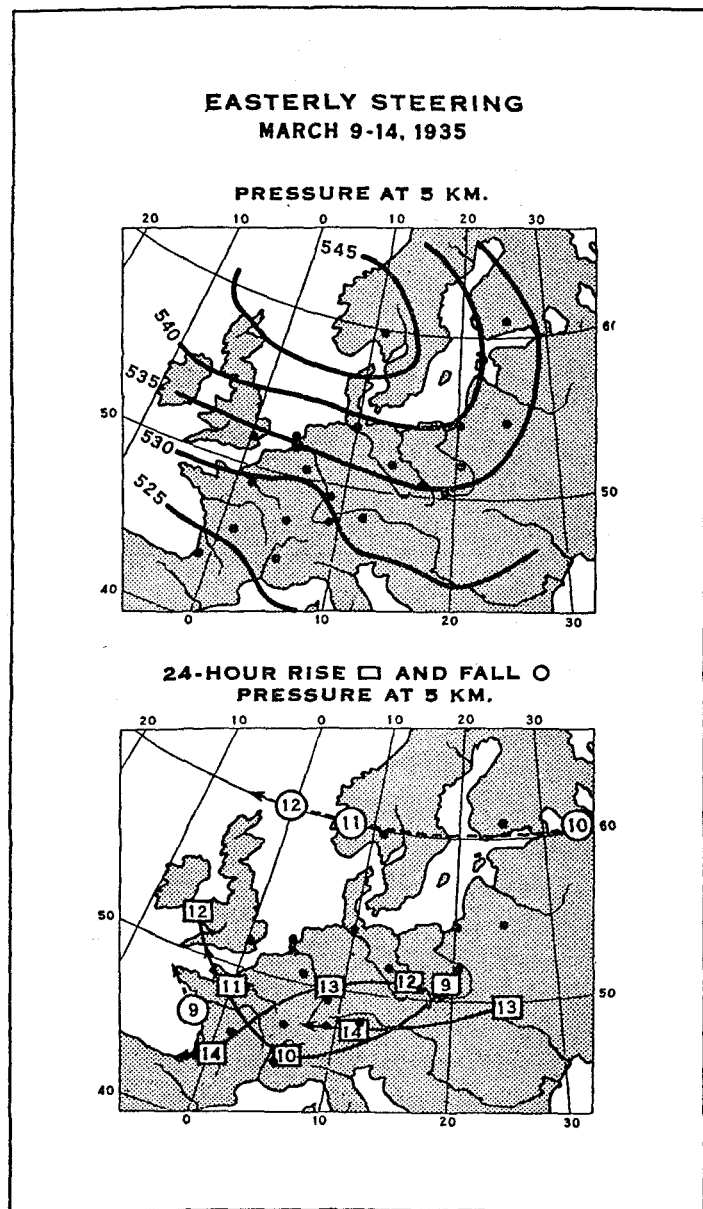
FIGURE 15.—Reproduced from Baur (10).

FIGURE 16.—Reproduced from Baur (10).

the neighboring countries (reference made to maps prepared at Bergen); any wave formation must have had its origin higher up. A case of trough steering is also shown (fig. 17). The representativeness of the above examples is shown by the fact that in 14 out of 15 periods composing the first 3 months of 1935 the steering principle worked. In the other case there was no definite direction of motion. The other three-quarters of 1935 and the year 1936 showed similar results.

In cases where the pressure gradient in the lower strato-

which, however, is not strong enough to affect the lower layers. Because of the different pressure distribution in the middle troposphere the flow there and below it is also different. This is indicated from the winds at 2 and 4 kilometers as well as from the movement of the 3-hour pressure-change fields.

The fundamental state (Grundzustand) essentially determines the broad-weather situation and can be identified by the pressure gradient in the lower stratosphere, by the temperature gradient in the troposphere, by the general flow and by the direction of motion as well as velocity of the 24-hour pressure fall and pressure rise fields (steering). The fundamental state, naturally, is of a

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TROUGH STEERING
MAY 17-20, 1935

PRESSURE AT 5 KM.

PRESSURE AT SEA LEVEL

24-HOUR RISE ☐ AND FALL O
SEA-LEVEL PRESSURE
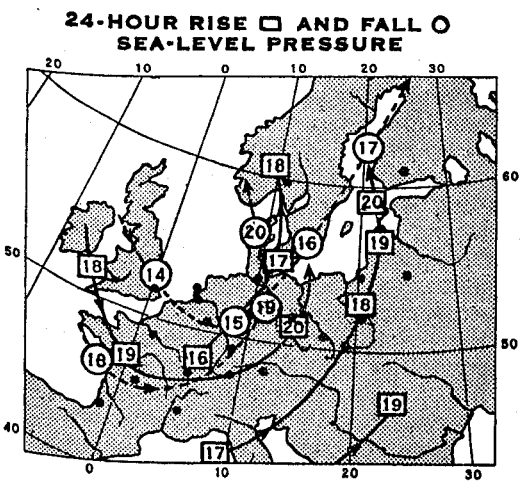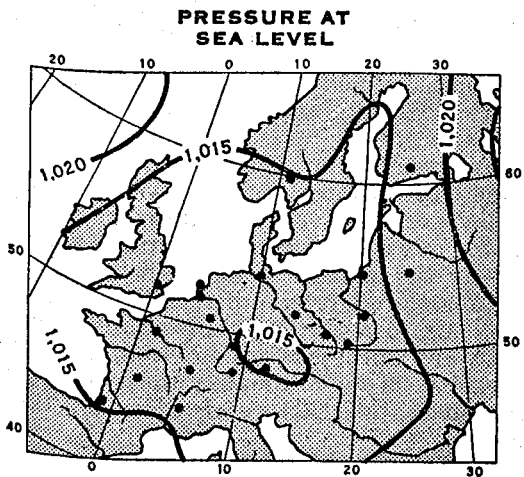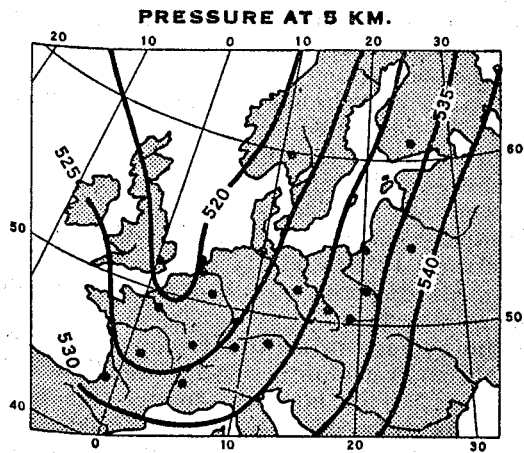
FIGURE 17.—Reproduced from Baur (10).

DOUBLE STEERING
FEBRUARY 16-22, 1936

PRESSURE AT 5 KM.

3-HOUR RISE ☐ AND FALL O
SEA-LEVEL PRESSURE

24-HOUR RISE ☐ AND FALL O
REGIONS OF PRESSURE* AND WINDS△
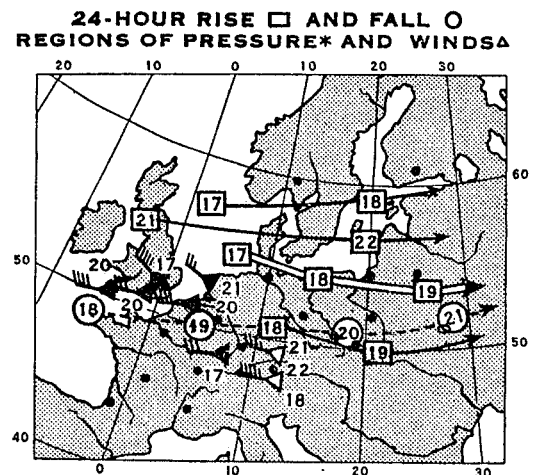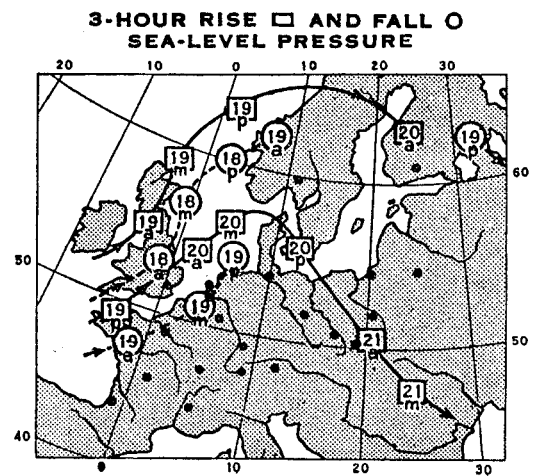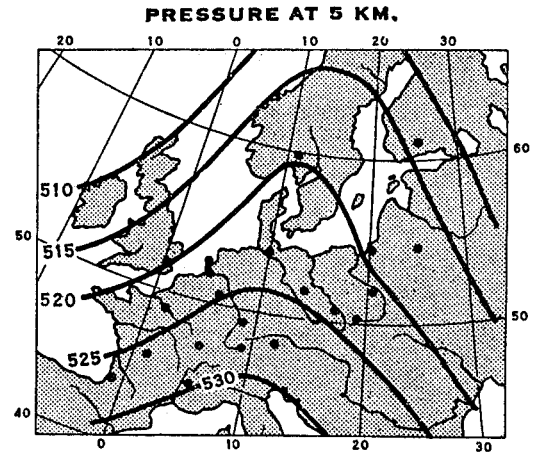
* Double lines for sea level. single lines for 5 km.
▲ Winds at 8 km.   △ Winds at 10 km.

FIGURE 18.—Reproduced from Baur (10).

longer duration than a perturbation which lasts about half a day or a day. Since the four elements enumerated above are mutually related at least in a general way, one of them could be employed in the determination of the

WINDS AT 2, 4, AND 8 KM.

WINDS AT 4 AND 8 KM.
MARCH 11 AND 12, 1935

WINDS AT 4 AND 8 KM.
MAY 17-20, 1935

WINDS AT 2 AND 4 KM.
FEBRUARY 16-22, 1936

▲ *Winds at 2 km.*　　● *Winds at 4 km.*　　○ *Winds at 8 km.*
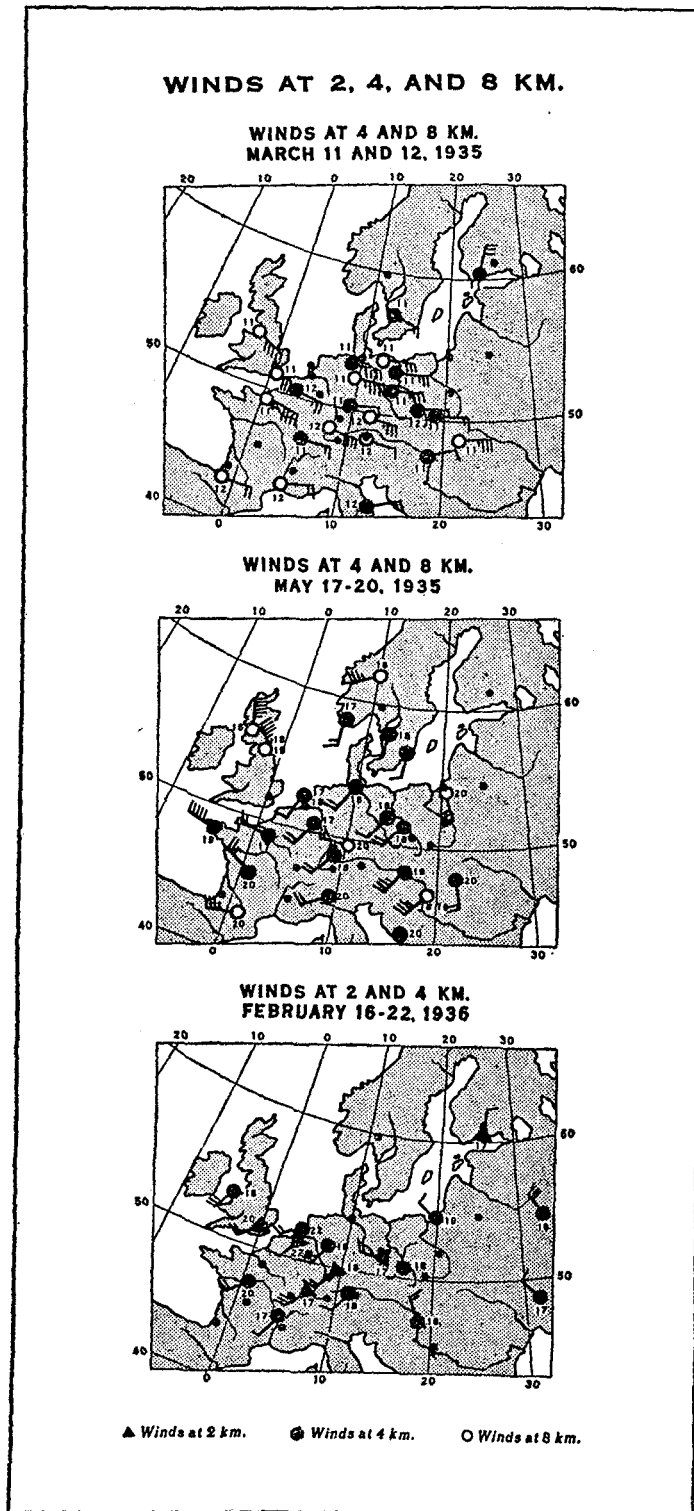
FIGURE 19.—Reproduced from Baur (10).

broad-weather situation. Only in case of double steering the situation is more complicated and necessitates the knowledge of the two controls, that is the knowledge of the pressure gradient in the lower stratosphere as well as that of the lower level (5 kilometers), from which it differs.

Often there are days during which the broad-weather situation is in a state of transition or transformation. These are days on which the pressure distribution aloft, or the temperature in the troposphere, is essentially dissimilar to the pressure distribution on the preceding day or subsequent days, also, when there is no mutual relation between the four elements of the broad-weather situation. These departures from the normal state are of deep significance in forecasting the further development of the broad-weather situation.

The duration of a broad-weather situation in central Europe can be measured by determining the length of periods during which the pressure distribution at say 5 kilometers, especially with regard to the pressure gradient remains essentially the same.

Baur's experience (10) over a period of a year and a half (written in early 1936) was—

1. The mean duration of a broad-weather situation is 5½ days.

2. On the average in central Europe there are five of these situations in a month, the intermediate days being transition days. This, however, does not mean that there cannot be a recurrence of the same broad-weather situation.

3. The longest durations are of west steering when central Europe is under a region of high pressure in the stratosphere and is, therefore, not crossed by areas of rise and fall in pressure.

4. The most frequent directions of steering are west, 18 percent; northwest and southwest, 17 percent each; quite rare is southeast; rarer still, northeast and north.

The above results show the importance of the pressure distribution aloft in forecasting the weather trend during 5- to 10-day periods in central Europe. The formation and breaking down of stratospheric HIGHS and LOWS is as important for the weather trend as the surface LOWS and HIGHS are for the daily weather. The departures from the normal stratospheric pressure gradient come about through strong outbreaks of air from the subtropical high-pressure belt, and ultimately must be dependent on the variation in the balance of radiation through the atmosphere.

8. METHOD UNDERLYING THE MAKING OF THE FORECASTS

The working foundation underlying the actual method of preparing the 10-day forecasts is mainly synoptic meteorology and statistics. The indications forming the basis of these forecasts are obtained in two different and independent ways. The results are then compared with each other for the purpose of reaching a mutual estimate of the coming weather type. In the final estimate all *known facts and additional indications which may have a bearing on the future weather development* are taken into consideration.

The working procedure consists of two parts, first a determination of the connection between the preceding and coming weather (it is thereby assumed that the atmospheric events of the preceding 10 days insofar as one considers them over a wide region determine, in main, the weather of the next 10 days), second, the individual study and analysis of the broad-weather situation on the day of the forecast. The former involves the extraction, from the weather history, and with the aid of multiple correlation tables, of definite days on which the weather development was similar to that of the forecast day. These broad-weather situations are critically analyzed statistically and synoptically to ascertain the probable future weather trend. Also the possibility of extrapolation from rhythms

existing at times in the weather is considered, especially in pressure, appearing up to the forecast day. However, since the periods of these rhythms change abruptly, their significance in forecasting must be studied in each case.

The first step in the statistical phase of the method is the determination of relationships between the preceding and coming weather trend. No regression equations are used; only multiple correlation tables. The latter, according to Baur, express much better the manifold connection and its underlying physical significance than the regression equations, and form the high point of the statistical method.[17] The statistical treatment of past weather makes up the principal part of the work. But once accomplished it is available for subsequent use as well.

With regard to the role of the broad-weather situation the hypothesis is made that the direction and magnitude of the pressure gradient aloft are important criteria in the determination of the character of the general weather. The upper pressure gradient can be determined from surface observations through (1) running means covering several days which eliminate the smaller fluctuations of tropospheric origin, (2) simultaneous consideration of pressure and temperature; especially changes in these elements if such exists, (3) interdiurnal variability of pressure at the surface, and (4) directly from upper air observations when these become available.

Thus with the aid of statistics a broad picture of the synoptic weather situation is obtained.

The basis for forecasting is a computation of correlations between foreweather and postweather, the drawing up of maps and the construction of tables. Thus for a given decade, say the second in July (11th to 20th), 10 overlapping foredecades are used, June 26 to July 5, June 27 to July 6, etc., and July 5 to July 14. The last day of each of these decades is called the indicator day (*stichtag*). The decade following each foredecade is called the postdecade. Every correlation between foreweather and postweather is calculated for each of the 10 pairs of decades so that for a 40-year period as a basis, a total of 400 (440 for the last decade in July and August) pairs must be prepared for each element for each forecast period. This must be done, Baur says, because it is essential in correlation researches to deal with the totality of all cases through the use of overlapping means. In computing the errors of the statistical measures, it must be remembered that the pairs of values are not independent of one another.

The consideration of the statistically derived relationships between the foredecade and postdecade beginning with the 5 days preceding and up to the 5 days following the day of the forecast, tells which of the correlations are the most significant and therefore which of them are of greatest utility.

The following elements of the foreweather serve as a foundation in the correlation computations:
1. The mean pressure of the foredecade.
2. Change in the pressure during the foredecade (difference between the mean of the first 5 days and that of the last 5 days).
3. Change in temperature in the foredecade.
4. Change in pressure in the last 5 days (from the morning of the 5th to the morning of the 10th).

5. Pressure on the morning of the last day (forecast day).
6. Interdiurnal variability of pressure in the last 6 days.

These meteorological factors are computed for 26 European and near European stations, the most westerly being Angmagsalik, the most southerly Ponta Delgada and Algiers, easterly Kiev, and northerly Vardo. In addition to these the pressure at certain points at sea, where meridians cross parallels of latitude were used. Also the temperature differences between the ocean and the continent. In order to exclude short oscillations the observations are combined into 5-day means. The elements of the postweather are: (1) mean pressure of the postdecade at (a) Potsdam; (b) Oslo; (c) Treuburg; and (2) precipitation frequency in the postdecade in (a) northern Germany; (b) southern Germany.

The computed correlation coefficients (point correlations) for each one of the stations and forecast points between the foreweather and the postweather are then entered on maps and brought plainly into view by drawing isocorrelates. From this one obtains the first inkling of the reliability of the suspected or even unsuspected relationships. The distribution of the correlations, the shifting of the centers of the greatest positive or negative correlation from decade to decade in some instances, and their persistence in a given region in others, is very significant in understanding the physical-synoptic background of the results. The movement of correlated areas indicates that it is not feasible to make reliable pronouncements about the coming weather entirely on the basis of synoptic weather analysis and preceding weather development by using standard rules applicable during an entire season or year. The factors which influence the broad-weather are different at the beginning of July from say in the middle of or toward the end of August.

In working up the relationships between past and coming weather the computation of combined instead of point correlations was resorted to. This involves the combining of values from several stations for each foredecade.[18] By means of these combinations a greater significance of the correlations and a stronger prominence of the physical-synoptic interrelations were attained.

Finally the quantities which were thus recognized as factors indicating the past weather trend were set into multiple correlation tables with one of the elements of the following weather. Baur says: "Such multiple correlation tables—mostly with four entries, i. e. variants of the foreweather were computed eight for each decade." The maximum values of the correlation ratios computed up to now are 0.82 to 0.89. These multiple correlation tables allow the evaluation not only of the closeness of the relationship between definite values of the preceding weather and the following weather development but from them one can also pick out the days of the preceding years on which an essentially similar weather situation to that of the day on which the forecast was made prevailed. This is very important since it is not sufficient to know and consider the mean relationships over a 40-year period. One must consider also the single, individual weather situations of the preceding years. Therefore for each year and for each day maps are drawn which can be consulted when making the forecast so that they could be compared with similar weather situations. Thus by means of a simple code giving the date of each event noted in the multiple correlation table one can immediately search out,

[17] Baur objects to the common practice of "setting ascertained correlations in simple, planned, linear rows." He says: "In meteorology it happens in most cases that a quantity A shows an entirely different connection with another quantity B according as a third quantity C and perhaps a fourth D show this or that anomaly." For example, the existence of a west-east temperature gradient is of varying significance for the coming weather according to whether at the same time a south-north or a north-south pressure gradient exists. In the multiple correlation tables the natural laws of dependence get full value.

[18] The methods of combination and selection of appropriate stations were determined from meteorological considerations and from the distribution of correlation coefficients.

on the day a forecast is to be given, all the past cases in which the variables had the same values as at the present.

In order now to be in position to compare the present weather conditions with similar events in the past and to be able to consider the effects of certain deviations from this, the six elements of the foreweather listed above are cartographically represented for every single indicator day and a comprehensive, exhaustive choice of observational facts is placed opposite the postdecade. In making the comparison it is noted that in every year the dates of the decades are the same. For the four years treated up to 1937 there are 4×6 or 24 decades (July and August). However, since it is desirable to avail oneself of all possible information (in a 10-day period the broad-weather situation may change) each of the 62 decades represented by the 62 days comprising July and August is considered separately. Thus we have for the 4 years 248 decades. A still firmer basis is sought by Baur and for each of these possible decades he makes the comparison of the postdecade with each day of the foredecade, 10 in all. Thus a total of 2,480 sets of charts is prepared. Each set compares 6 charts of the conditions in the foredecade with 4 charts of the postdecade accompanied by 4 tables giving information for each of the 10 days regarding air-mass type, maximum temperature, duration of sunshine and precipitation.

Thus for each single day of the decades comprising the four years in question there will be accordingly arrayed opposite each other:

Weather of the foredecade

1. Map of the mean departure of pressure from normal in the foredecade.
2. Map of the change in the mean pressure from the first to the second pentad of the foredecade.
3. Map of the change in the mean midday temperature from the first to the second pentad of the foredecade.
4. Map of the change in pressure in the last 5 days.
5. Map of the interdiurnal variability of pressure in the last 6 days.
6. Map of the distribution of pressure on the morning of the last day of the foredecade.

Weather of the postdecade in Germany

1. Maps of the mean departure of pressure from normal for first and second pentads of the postdecade (including southern Scandinavia).
2. Maps of the mean departure from normal of the daily mean temperature of the first and second pentads of the postdecade.
3. Tables of the air mass types prevailing on each of the 10 days in the postdecade at Karlsruhe, Potsdam, and Treuburg.
4. Table of the daily maximum temperature at the characteristic stations.
5. Table of the daily duration of sunshine at characteristic stations.
6. Table of the daily precipitation at 30 evenly distributed characteristic stations in Germany.

These statements of the foreweather and postweather, for all the 2,480 days, are on file. The individual tabulations of the elements for each day of the postdecade make possible reference to the past weather and aid the forecasting of weather changes within the 10-day periods.

4. PREPARATION OF FORECASTS

Preparation for the issuance of the forecast is made at the beginning of the foredecade. On the morning of the day on which the forecast is to be issued, the material of the first 9 days is prepared. With the arrival of the morning reports at 11 o'clock all the observations are worked up statistically and 6 charts of the previous weather are finished by 1 p m. By this time the data for the multiple correlation tables are also prepared. The mathematical expectation of the mean pressure for the postdecade at Potsdam, Treuburg, and Oslo as well as rainfall frequency for north and south Germany is then computed. In this way the first idea about the weather trend for the coming forecast period is obtained. Simultaneously the charts for the forecasting day are drawn up. With the aid of the multiple correlation tables all weather situations which occurred during the 40-year period and which show a similarity with the instantaneously prevailing weather situations are searched out. These instances are each singly compared with the present advance weather, and the useless cases rejected. Then as a rule there remain 2 or 3 cases which on all the 6 charts picturing the previous weather show approximate agreement with the chart giving the current weather situation. From these similar instances the maps of the following weather, including the 10 maps of the postdecade are closely studied and synoptically analyzed with the view of determining the most probable current weather development. At the same time close attention is paid to the manner in which the prevailing weather situation differs from the older one. Meanwhile the noon reports are examined so that the charts giving the change of the mean noon temperature from the first to the second pentads of the foredecade can be obtained and weighed for further similarities. In the synoptic analysis the last and by no means minor criterion for the expected weather situation is found in the upper air conditions on the day of the forecast and on the day which precedes it. In addition, rhythms in the preceding pressure, which already appear to some extent in the variability charts, were studied at a few especially important stations. The final decision regarding the general weather situation is reached at a conference held by the director of the institute with his assistants. The wording of the forecast is personally composed by the director at about 6 p. m.

As an example of the wording of a forecast the announcement for the third July decade of 1935 is quoted:

The fair, prevailingly dry weather of the previous week changed on the 15th to somewhat unsettled weather, but nevertheless on the whole the weather especially in south Germany retained a friendly aspect.

This not unfriendly but, on the other hand, also not fully settled weather with alternate clearing and short, partly thundery rains will continue for the next few days. Then in the west and in south Germany for a few days prevailingly clear and dry weather will come, while in north Germany, especially in the coast region and East Prussia a slight changeableness will remain. Following that over the whole country there probably will be another cooling and strongly unsettled weather with frequent precipitation due to arrival of maritime and polar-maritime air currents.

In the first half of the last third of July the temperature as the result of a sharp change will come to have a practically normal mean. In the second half on the whole it will be cooler than normal. The number of days with precipitation in the last third of July in central north Germany and East Prussia will exceed 5 in most places though on many days the showers will be merely insignificant. The duration of sunshine in south Germany in the 11 days will exceed 70 hours.

Baur says:

This forecast was well fulfilled; however, the number of rainy days in central north Germany in many places was only 5 instead of more than 5. Especially impressive was the arrival of the forecast cooling from maritime and polar-maritime air for the second half of the decade.

It is worth noting that to one who is accustomed to deal with figures the second paragraph may appear to be more definite than the first; the farmer and layman in general will probably find the first paragraph more informative. The writer agrees with Walker (loc. cit.), who in referring to the above forecast says: "Such a statement is by no means lacking in definiteness."

5. DISCUSSION AND CONCLUSIONS

The essence of Baur's method of forecasting lies in the determination of the particular type of broad-weather situation or situations, both with respect to type and duration. This, we saw, is achieved through a study and analysis of the broad-weather situation of the day on which the forecast is issued. The latter involves the extraction from the weather history, with the aid of multiple correlation tables, of definite days when the weather development was similar to that of the forecast day. The broad-weather situations obtained in this manner are analyzed statistically and synoptically to ascertain the probable future weather.

If there is any connected sequence in weather, as we know there is, this sort of treatment should bring it out and, therefore, supply a powerful tool for the application of past experience. The justification is that the weather has behaved thus and so in the past and since it is governed by physical laws, whether or not yet known, the weather may be expected to follow a similar trend from like beginnings at another time. The prudent and skillful employment of statistics predicated on an understanding of the problem can also be counted upon to give a valuable indication of the future weather.

The above considerations are an indication that Baur's method of forecasting the weather trend for 10-day periods during the summer months is capable of giving valuable forecasts.

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VERIFICATION OF BAUR'S TEN-DAY FORECASTS

By Larry F. Page and Philip F. Clapp

As part of a general study of the work of Dr. Franz Baur in the field of long range weather forecasting it was decided to verify some of his forecasts. Those chosen for verification were his 10-day forecasts which were issued during the spring and summer, from 1933 to 1936. In 1933 the forecasts were issued for successive 10-day periods from July 6 to August 25. In following years the period covered by all the forecasts was increased, until in 1936 they covered the interval from June 18 to August 29. Also, in 1936 the forecasts were issued on the same day of each week, so that each overlapped the preceding one by 3 days. There were thus 27 ten-day forecasts to be verified.

These forecasts are of the usual descriptive type, but since they concern the three elements temperature, precipitation, and hours of sunshine, and since in many cases they refer to different parts of Germany, it was found that for each 10-day period there were several more or less independent forecasts. To give a concrete example, we may consider the fifth forecast of 1936, covering the period Thursday July 16 to Saturday July 25:

The unsettled weather which has been prevailing in Germany since about a week will continue in the following days. With it one is to expect in general cooler weather with variable cloudiness, and daily rains.

Around the end of the week an improvement in the weather will take place. Rains will diminish, daily duration of sunshine and temperature will increase again.

The improvement which apparently will come somewhat later in the northeast than in the rest of Germany will last only a few days. Then unsettled, yet not entirely unfavorable, weather will come again. The rains will be mostly of the thunderstorm variety.

The total duration of sunshine during the 10 days will be in most places between 50 and 80 hours. The number of rainy days will be more than 5 almost everywhere with the possible exception of the northeast. The temperatures on the average will be below normal.

Here we may distinguish the following separate forecasts:

(1) Unsettled weather with daily rains will continue until "around the end of the week." Thus, rainfall frequency will be above normal.

(2) This will be accompanied by temperatures below normal.

(3) An improvement which will bring decreased rainfall will occur "around the end of the week," but somewhat later in the northeast than in the rest of Germany. This improvement will last "only a few days."

(4) Temperatures will be higher during this improvement.

(5) After this the weather will be unsettled.

(6) The total hours of sunshine during the 10-day period will be between 50 and 80.

(7) The number of rainy days will be more than 5 with the possible exception of the northeast.

(8) The mean temperature during the 10 days will be below normal.

Thus, for this particular 10-day period there are 8 forecasts covering all three elements and various periods from about 3 to 10 days. It was decided as the first step in verification to analyze each of the forecasts in the above way. It will be noticed that the period covered by the forecast is often rather indefinite. This was found to be true throughout the forecasts. However, if the forecasts are to be of any use, one must guess at the limits of these periods, so it was thought that we were perfectly justified in doing so. There are also certain ambiguous statements which we decided to omit, such as that the weather will be "on the whole not unfavorable." Statements of the amount of cloudiness were also omitted, as it was felt that forecasts of the duration of sunshine were equivalent, as well as being simpler to verify.

Our original plan was to test the forecasts by calculating the probability that each be right by chance, and then, using the same probability, by making a forecast of the same factor based on the normal values of the elements. We could then require that in order to be useful the forecasts have a better mark than the normal "forecasts," and that those of the same probability have a percentage accuracy which is greater than this probability. In other words, they must have a mark which is better than that which we would expect by chance. Unfortunately, because of the limitations of the data and the nature of the forecasts, it was necessary to modify our original plan to fit each particular type of forecast.

Before describing the methods used in verifying each type of forecast, a brief account will be given of the sources of data and stations used in the verification. To represent the various sections of Germany we used 9 stations for temperature and sunshine. These were Königsberg, Berlin, Breslau, Bremen, Aachen, Frankfurt a. M., München, Karlsruhe, and Wahnsdorf. For precipitation we used 15 stations, namely Königsberg, Berlin, Breslau, Hannover, Hamburg, Bremen, Aachen, Frankfurt, Karlsruhe, Nürnberg, München, Kiel, Magdeburg, Stettin, and Wahnsdorf. Data for these stations were obtained from the following records: Tägliche Beobachtungen and Niederschlagsbeobachtungen of the Deutsches Meteorologisches Jahrbuch. These records were mostly for the years 1934 and 1935. Before 1934 and for 1936 we used the separate year-books of the German Central Stations and the Tägliche Wetterbericht of the Deutsche Seewarte. This last source was found to be very faulty as regards precipitation records, so that to avoid using it necessitated the use of only 7 precipitation stations in 1933 and 11 in 1936.

When each of the 27 ten-day forecasts had been analyzed as previously described, it was found that there were 152 separate forecasts of temperature, precipitation, and sunshine. These forecasts are of several different types, and as each type had to be treated separately, they will be discussed below.

Of a total of 51 temperature forecasts, 32 are forecasts of the departure from normal or of the range within which temperatures will lie. Such a forecast might read, "Predominantly warm weather will prevail in the following days in south Germany." The probability was assumed to be 50 percent for a forecast of departure above or below normal. In order to test a normal forecast by using the range about normal which will include 50 percent of the cases, we must use some criterion of variability such as the standard deviation. The same is true

of the test forecasts based on normal temperatures. The standard deviations and normals for each station were calculated from 40 monthly means in the case of all but 1 of the 9 temperature stations. For this station, Wahnsdorf, only 20 years of record could be obtained. Normals for any number of days from 1 to 10 were taken from a smooth curve drawn through the monthly normals of May to September. Calculation of the standard deviation was based on the assumption that the distribution of temperatures about the average is a normal one. Thus we first found the standard deviation of the mean temperature of each of the 3 months June, July, and August, from the 40 years of record. Assuming that successive days of each month are independent of one another, the mean standard deviation for each day was obtained by multiplying the monthly deviation by $\sqrt{30}$. The deviation for any number of days was then obtained by dividing the result by the square root of the number of days concerned. As this process involves the assumption that the standard deviation does not change from day to day in a single month and that the temperatures of successive days are not dependent on one another, any temperature range or probability calculated by this means is obviously no better than approximate. This error tends toward a higher percentage verification of the normal forecasts than would otherwise be expected. But since, as will be shown later, we only require that the probabilities be right within 10 percent, we feel justified in using them.

There are 11 forecasts of a change of temperature during the forecast period. A typical forecast would be, "Cooler weather will set in toward the end of the week." The probability of getting this forecast right by chance was considered to be 50 percent. No attempt was made to check this forecast with one based on normals, since changes in normal temperatures in the brief periods involved are seldom greater than 0.2 degree centigrade, and it was not felt that a forecast based on such a slight change is justified.

There are three forecasts of a change of temperature at the beginning of the forecast period, as, "In the following 10 days cooler weather will prevail." The probability of getting this type correct by chance depends on whether the temperatures previous to the forecast are above or below normal, because obviously if temperatures are already above normal, chances are much greater than 50 percent that they will be lower in the following period. The probability is calculated by using the standard deviation together with a knowledge of the previous temperature departure. As in the previous type, no normal forecast is made.

The final type of temperature forecast, of which there are five cases, is one in which a difference in temperature between two regions is predicted, such as, "Temperatures will be cooler in south Germany than in north Germany." Here we made no attempt to calculate the probability or to test it with a normal forecast.

Of the 90 precipitation forecasts 13 are forecasts of the number of days of measurable precipitation during the forecast period. Such a forecast might read, "The number of rainy days in the greatest part of Germany during the period of forecast will be between 4 and 6." The probability cannot be calculated in the same way as for the first type of temperature forecast, by use of the standard deviation, because in this case we are not justified in assuming that the distribution about the average value will be a normal one, because the distribution has the definite upper and lower limits of 10 and 0 days and because it is a discontinuous distribution. The proba-

bility can be calculated, however, by constructing the actual frequency distribution for as long a period as is available. We were able to get 25 years of record for most of the 15 stations and 22 years for the others. We assumed that the distributions and normals do not change appreciably throughout a single month, so that by using the data for each of the 3 decades there were between 66 and 75 cases available for the construction of each frequency distribution. The probability for a single forecast of say 4 to 6 days is estimated by finding the relative number of decades in which there were 4, 5, or 6 days of rain. Test forecasts are made by taking the days on either side of the normal value which will include approximately this same proportion of cases. Probabilities calculated in this way are only approximate, in view of the shortness of the records.

There were two forecasts of the departure of precipitation from a 10-day normal, as, "In the next 10 days quite unsettled and frequently rainy weather will prevail." If we assume that this means that the number of days of appreciable rainfall rather than the amount of rainfall, will be above or below normal, this can be verified in precisely the same way as in the case of the first type of precipitation forecast. The probability is 50 percent and the normal forecast is given a range which will include as nearly as possible this proportion of cases.

The most numerous type of precipitation forecast, and of which there are 48 cases, is that which gives the departure from normal for a period which is less than 10 days, as, "Unsettled weather will prevail in the next 3 to 5 days." This type has a probability of approximately 50 percent, and as in the case of the preceding type is taken to mean that the number of days of appreciable rainfall will be above or below normal. However, in this case a normal forecast cannot be made based on the same probability, since no frequency distributions were constructed for a period differing from 10 days.

There are 18 forecasts of a change in precipitation during the period of forecast, as, "After 4 to 6 days the weather will be more unsettled and more rainy." This forecast has a probability of 50 percent of being right by chance. The verification is made by comparing the average daily amounts of precipitation during the 2 periods involved. This was thought to be more significant than the average number of days of rainfall, especially for short periods. No normal forecast was made, since it was assumed that there is little change in normal precipitation throughout a single month.

The final type of precipitation forecast, of which there are nine cases, is one giving the difference in precipitation between two regions, as, "The rainfall will probably occur more frequently in north Germany east of the Oder and in Silesia than in the west." No probability can be assigned to this type of forecast, nor can a normal test forecast be made.

There is only 1 type of sunshine forecast, of which there are 11 cases. These forecasts give the total number of hours of sunshine during the forecast period, or the range within which the total duration will lie, as, "The total hours of sunshine during the 10-day interval will amount to at least 60 almost everywhere." The probability of such a forecast can be estimated by use of the standard deviation, as in the case of the first type of temperature forecast. The necessary normals, for a 25-year period, were obtained for the nine stations from a climatological atlas of Germany, but unfortunately long enough records for a computation of the standard deviation were available only in the case of two or three stations. It was necessary

to assume that the standard deviation for a given month was the same for all nine stations, and this assumption was justified by the close agreement between the standard deviations of a 32-year record for Bremen with those of a 46-year record for Karlsruhe. Those for Karlsruhe were used since they are based on a longer record. Probabilities calculated in this manner are of course only approximate.

To obtain a general estimate of the accuracy of the forecasts it was necessary to combine the results of the verifications described above. In order not to weight the forecasts by the number of the stations used in verifying them, we gave each forecast a total weight of 1. The rating given any forecast is then the number of stations for which the forecast was correct divided by the total number used in that particular forecast. For example, if a temperature forecast used all 9 of the temperature stations and 6 of them were found to conform to the forecast, it would be given a rating of 67 percent. The forecasts may now be totaled in any convenient way. Due to the approximations involved in computing the probability and ranges used in verifying some of the forecasts, and due to the great number of different probabilities found in the results, we decided to sum the forecasts in probability groups with a class interval of 10 percent. Thus, all forecasts whose probabilities lie between 45 percent and 54 percent are in the same group, etc.

The results of this grouping are shown in the accompanying tables.

Table 1 shows the total results of the forecasts for each element and each year. It was found on summing the forecasts by probability groups that only in the case of the 45–54-percent group for each of the elements, and the 35–44-percent group for sunshine were there enough cases to consider the results significant. Therefore these three groups, together with the total of all groups, are the only ones included. Thus, the two rows in temperature and precipitation refer to the 45–54-percent group and the total of all groups. The three rows in sunshine refer to the 35–44-percent group, the 45–54-percent group, and the total. The three rows representing the sum of all three elements refer to the 45–54-percent group, all other groups, and the total. The 4 columns show the number of forecasts correct, the total number of forecasts, the ratio of the first two columns expressed in percent and the probability of being correct by chance. The probability assigned to the total of all probability groups is obtained by finding the weighted mean of the groups, weighting the central value of each group by the number of forecasts it contains. Since each group has an interval of 10 percent the weighted mean should be considered accurate to not less than 5 percent. All percentages based on more than 10 forecasts are given in italics, as these have the highest significance. Although a large proportion of the percentages are not shown in italics, it should be noted that out of a total of 48 percentages all are above the corresponding probabilities. This means, of course, that the forecasts have a much higher mark than we would expect by chance, even considering that the percentages are not all independent. Another point to be noted is that 1934 has the best record while 1936 has the worst. The comparatively poor record in 1936 is due to the poor temperature forecasts of that year. 1933 is also a poor year for temperature forecasts, while the mark for 1935 is lowered by poorer precipitation forecasts.

Table 2 compares the results of those forecasts which could be tested with normal forecasts with the results of the corresponding test forecasts. Unfortunately only 38 percent of all the forecasts could be tested in this way, so that the conclusions that may be drawn from these tables regarding the forecasts as a whole are limited. The rows refer to the same groups as in the first set of tables. The first four columns contain the number of forecasts right and the total number for both the normal forecast and the actual forecast. The last three columns contain the corresponding percentages and the probability. Because of the lesser number of forecasts contained in these tables there are many fewer percentages in italics than in the case of the first tables. However, it should again be noted that only 2 out of 48 forecast percentages are lower than the probabilities, whereas this is true in 12 cases for the normal forecasts. In 8 of the 50 comparisons the forecasts have a lower mark than the corresponding normal forecasts, but only 4 of these are based on a sufficient number of cases to consider the results significant. These 4 unfavorable comparisons are due to the comparatively poor temperature forecasts of 1933 and 1936, which have been mentioned before. However, it is only fair to state that the probabilities of the normal temperature forecasts are undoubtedly higher than those indicated because of the assumptions made in estimating the standard deviations.

TABLE 1.—*Forecast totals*

	1933				1934				1935			
	Forecasts correct	Number of forecasts	Percent correct	Percent probability	Forecasts correct	Number of forecasts	Percent correct	Percent probability	Forecasts correct	Number of forecasts	Percent correct	Percent probability
Temperature	4.70	7.50	63	45-54	6.81	9.00	76	45-54	9.67	12.00	81	45-54
	5.37	9.00	60	48	7.81	11.00	71	53	10.67	14.00	76	50
Precipitation	7.10	10.10	70	45-54	5.63	7.67	73	45-54	14.15	25.47	56	45-54
	10.20	14.00	73	50	8.67	13.00	67	53	16.17	29.00	56	50
Sunshine	1.00	1.00	100	35-44	0	0	----	35-44	.58	.91	64	35-44
			----	45-54	.44	.44	100	45-54	.25	.25	100	45-54
	1.00	1.00	100	40	1.66	2.00	83	65	2.67	3.00	89	51
Sum of three elements	11.80	17.60	67	45-54	12.88	17.11	75	45-54	24.07	37.72	64	45-54
	3.77	5.40	70	44	3.09	3.89	79	73	2.69	4.28	63	44
	16.57	24.00	69	49	18.14	26.00	70	54	29.51	46.00	64	50

	1936				Four-year sum			
	Forecasts correct	Number of forecasts	Percent correct	Percent probability	Forecasts correct	Number of forecasts	Percent correct	Percent probability
Temperature	8.34	14.00	60	45-54	29.52	42.50	69	45-54
	9.34	17.00	55	50	33.19	51.00	65	51
Precipitation	16.97	28.17	60	45-54	43.85	71.41	61	45-54
	22.03	34.00	65	50	57.07	90.00	63	50
Sunshine	2.68	4.23	63	35-44	4.26	6.14	69	35-44
	.11	.11	100	45-54	.80	.80	100	45-54
	3.12	5.00	62	37	8.45	11.00	77	46
Sum of three elements	25.42	42.28	60	45-54	74.17	114.71	65	45-54
	5.07	7.72	66	40	14.62	21.29	69	49
	34.49	56.00	62	49	98.71	152.00	65	50

A classification of the forecasts can be made from the point of view of the time elapsing between the issuing of the forecast and the period for which it applies. As pointed out before, each of the 10-day forecasts is made up of secondary forecasts which apply to different parts of the 10-day period. For example, a secondary forecast might begin on the 6th day of the forecast period and end on the 8th. The 152 forecasts were divided into 4 groups: (1) entirely within the first 5 days; (2) entirely within the last 5 days; (3) forecasts for less than 10 days which overlap the 5th day; (4) those which cover the whole 10-day period. There are 43 forecasts in the first group, and these have a percentage verification of 75. Those in the second group, consisting of 36 cases, have a percentage of 53. Those in the third and fourth groups each have a percentage of about 65. All of these groups have a probability of about 50 percent of being right by chance. It will be seen that the second group has a mark which is not sig-

nificantly greater than this chance percentage, which indicates that Baur's forecasts have little or no value, after the first 5 days.

TABLE 2.—*Comparison of normal test forecasts and Baur forecasts*

1933 and 1934

Element	Normal Correct	Normal Number	Forecast Correct	Forecast Number	Normal, percent correct	Percent correct	Forecast percent probability	Normal Correct	Normal Number	Forecast Correct	Forecast Number	Normal, percent correct	Percent correct	Forecast percent probability
Temperature	3.52	5.50	3.83	5.50	64	70	45–54	5.72	8.00	6.14	8.00	72	77	45–54
	4.69	7.00	4.50	7.00	67	64	48	5.72	8.00	6.14	8.00	72	77	50
Precipitation	1.20	1.81	1.32	2.10	66	63	45–54	.58	1.29	.91	1.47	45	62	45–54
	2.94	5.00	3.42	5.00	59	68	49	1.01	3.00	1.21	3.00	34	40	52
Sunshine	0	1.00	1.00	1.00	0	100	35–44	0	0	0	0	---	---	35–44
	0	0	0	0	---	---	45–54	.33	.44	.44	.44	75	100	45–54
	0	1.00	1.00	1.00	0	100	40	1.43	2.00	1.66	2.00	72	83	65
Sum of three elements	4.72	7.31	5.15	7.60	65	68	45–54	6.63	9.73	7.49	9.91	68	76	45–54
	2.91	5.69	3.77	5.40	51	70	45	1.36	2.27	1.35	2.09	60	65	66
	7.63	13.00	8.92	13.00	59	69	48	8.16	13.00	9.01	13.00	63	69	53

1935 and 1936

Element	Normal Correct	Normal Number	Forecast Correct	Forecast Number	Normal, percent correct	Percent correct	Forecast percent probability	Normal Correct	Normal Number	Forecast Correct	Forecast Number	Normal, percent correct	Percent correct	Forecast percent probability
Temperature	5.53	8.00	7.12	8.00	69	89	45–54	6.90	9.00	5.33	9.00	77	59	45–54
	5.53	8.00	7.12	8.00	69	89	50	6.90	9.00	5.33	9.00	77	59	50
Precipitation	.07	.07	.27	.47	100	57	45–54	.75	1.51	1.61	2.17	50	74	45–54
	.79	2.00	1.29	2.00	40	65	42	2.08	5.00	3.67	5.00	42	73	46
Sunshine	.33	.91	.58	.91	36	64	35–44	2.79	4.23	2.68	4.23	66	63	35–44
	0	.25	.25	.25	0	100	45–54	.11	.11	.11	.11	100	100	45–54
	.66	3.00	2.67	3.00	22	89	47	3.23	5.00	3.12	5.00	65	62	37
Sum of three elements	5.60	8.32	7.64	8.72	67	88	45–54	7.76	10.62	7.05	11.28	73	63	45–54
	.63	3.68	2.69	3.28	17	82	45	4.45	8.38	5.09	7.72	53	60	40
	6.98	13.00	11.08	13.00	54	85	48	12.21	19.00	12.12	19.00	64	64	46

Four-year sum

Element	Normal Correct	Normal Number	Forecast Correct	Forecast Number	Normal, percent correct	Percent correct	Forecast percent probability
Temperature	21.67	30.50	22.42	30.50	71	74	45–54
	22.84	32.00	23.09	32.00	71	72	50
Precipitation	2.60	4.68	4.11	6.21	56	66	45–54
	6.82	15.00	9.59	15.00	45	64	48
Sunshine	3.12	6.14	4.26	6.14	51	69	35–44
	.44	.80	.80	.80	55	100	45–54
	5.32	11.00	8.45	11.00	48	77	46
Sum of three elements	24.71	35.98	27.33	37.51	69	73	45–54
	9.35	20.02	12.88	18.49	47	70	45
	34.98	58.00	41.13	58.00	60	71	48

All the preceding discussions compare the actual forecasts to those based on chance or on normal values. Persistence forecasts, based on the assumption that the weather during the forecast period will be the same as that in the previous period, due to the conservation of weather processes, in some cases give a much higher percentage of verification than either chance or normal forecasts. Since they do not require skill in their application, the test of a forecast method should include a comparison of results with those of persistence forecasts. It should be noted that from the practical point of view forecasts of change are often of the greatest importance, and these cannot be made by persistence, whereas Baur's forecasts include both continuations and reversals of sign of departure.

Only temperature and precipitation departure forecasts were tested in this way, as these are the only types which can be made directly by persistence.

Of the temperature departure forecasts, 27 were treated in the following way: A persistence forecast was made by saying that the sign of departure during the forecast period would be the same as that on the day the forecast was issued. Such persistence forecasts attain a percentage verification of 69.4 percent, while the corresponding mark for Baur's forecasts is 70.5 percent. The difference of 1.1 percent is not significant, either practically or statistically.

In the case of precipitation, there were 20 forecasts of frequency departure which could be analyzed. In this case the persistence forecast was based on the assumption that the number of days of precipitation during the forecast period would be greater than normal if rain occurred on the last preceding day. Such forecasts received a mark of 57.0 percent compared with Baur's percentage of verification for the same 20 forecasts of 68.5 percent. Here, as previously, each forecast was graded according to the percentage of stations included which were forecast correctly. Thus, although the stations are not entirely independent, considerably more than 20 forecasts were graded, and Baur's grade is probably significantly better than the persistence verification for precipitation.

A PRELIMINARY SUMMARY OF THE MULTANOVSKI SCHOOL OF LONG-RANGE WEATHER FORECASTING

By I. I. Schell

Introduction.—The Institute of Long-Range Weather Forecasts, after 1930 a division of the Central Weather Bureau of U. S. S. R., was established in 1912, at the Central Geophysical Observatory. B. P. Multanovski, head of the institute until his recent death, was entrusted with the work of carrying on investigations to promote the development of long-range forecasting methods. Together with his associates he developed, in the course of many years, the methods which serve as the basis for forecasts.

The first (1920) regular forecasts for 10 to 14 days to 2 to 3 months in advance which were issued were of an experimental nature. After 1932 they were issued in the same manner as the regular short-range forecasts. ¡The forecasts also extended for longer periods in advance. Under certain conditions they were issued for as much as five months ahead.

The time interval for which the forecast was generally issued was the period of time which marks the type of synoptic process involved and was termed a natural period or natural time interval as distinguished from a calendar time interval. Inasmuch as the development of a synoptic process often varies with season and from year to year and in addition is a function of a number of synoptic factors, the time interval must necessarily vary too, and consequently the period of time for which the forecast is issued.[1]

Of the three available papers in which a partial exposition of the principles and methods is attempted, that by Tichomirov (4) contains the briefest and will be followed here mainly.

1. FACTS AND PRINCIPLES OF FORECASTING

The method, known best by the name of composite map method, differs from time-space correlation long-range forecasting methods and many others in that it is based on synoptic considerations.

To get a simple picture of the trajectories of pressure areas plotted on a map Multanovski grouped them according to their origin. He chose for his experiment the trajectories of the maxima. His reasoning was that the central part of a maximum forms, at any rate as a first approximation, the nucleus of high pressure and therefore, the trajectory of the maximum can be taken, as a first approximation, as the trajectory of the nucleus, in other words, the trajectory of the air mass.

In grouping the trajectories according to their origin Multanovski obtained three groups, one directed from the west, the second from the northwest quadrant and the

third from the northeast quadrant. He then set these on separate maps.

In examining the track of each individual maximum for a period of many years he discovered that in general the intrusion of maxima takes place along definite paths and that the tracks are concentrated, for the most part, in definite zones. These concentrations were selected and a single average axis called a median or normal was determined for each. Thus there was obtained a sort of skeleton or fan representing average trajectories which can be regarded as the typical positions of the tracks of maxima. It also appeared that the axis with a northerly component formed two distinct groups or fans, the point of intersection of the axes of the first group, center of fan, lying in the Arctic at 105° W. 86° N. (northwest of Greenland) and the other around Taimir (northwestern Siberia). The axes of the first group represented maxima entering Europe from the northwest, and were called the normal polar type, while the other fan represented average tracks of maxima moving into European Russia from the northeast (in winter also from the east) and were named the ultra-polar type. The anticylcones arriving from the west, region of the Azores, followed a rather narrow zone almost straight east, and their path was called the Azores normal. In addition to the main Azores group there are a few offshoots directed northeastward. The position of axes over certain regions calls for a completely determined type of weather in corresponding regions.

It was found that some of the axes are characterized by great stability. The movement of anticyclones along a single axis often continues for a long time. This property might be utilized in the comparison of seasons among themselves according to the distribution of axes, as well as for comparison of each season with typical axes. In the latter instance we can determine the degree of deviation from the normal. The change in direction of a trajectory, or degree of deviation from normal, serves as a definite characteristic of a single season or period.

2. THE COMPOSITE MAP

In the study of synoptic processes there is a necessity of characterising a long-term process with a few maps which will bring out its essential features. The use of mean pressure maps in the characterization of a synoptic process is helpful in the case of a fairly steady type process. However, with rapidly moving areas of low and high pressure and sharp changes from day to day mean pressure maps fail to reveal the essential details of the weather of individual days.

To gain an estimate of the distributions of pressure areas during certain developments, such as the movement of a maximum along a definite axis, the method was used of entering on a map with the aid of special symbols the centers of areas of high and low pressure and related developments—crests of high pressure, secondary minima (See accompanying the movement of the maximum.

[1] The terms long-range calendar time interval and long-range natural time interval require an explanation. In long-range weather forecasting the period of time covered by the forecast may be either a calendar unit—week, month, season, or year, or a natural time unit, i. e., a time interval which is determined from meteorological considerations, whose length is governed by a particular synoptic process as represented by an individual weather situation or a continuous series of similar situations. In the latter case a physical picture is involved.

In some instances the calendar time interval may approximate or equal the natural period. In certain regions, India for example, where the weather is mainly determined by land or sea monsoons, and hence where the weather situation remains essentially similar for a long time, or in regions where there is little variation in the weather, the distinction between calendar and natural periods is obliterated.

fig. 1.) The maps obtained in that way retain all the details of the life history of the pressure regions, and no matter how long the period may be they reflect to a certain extent the weather of every day. In this respect they have a distinct advantage. We get for example an accumulation of cyclonic centers, and this shows that the conditions for the development of cyclonic activity in a given region were at hand; if in due course there were to appear in this region signs of partial minima and crests of high pressure it would serve as an indication that conditions would become unsettled. The regions of accumu-

state of one or several fields will serve as an indication of the change in the orientation of the axis and hence the termination of conditions favorable for the realization of a certain weather phenomenon; and conversely a deviation from the normal trajectory implies a change in the pressure field.

The construction of a composite map is of significance then for any well-defined process. The relationship between the orientation of the process, in part between the movement of the maximum along a definite axis and state of pressure field on the composite maps is often utilized in

COMPOSITE MAP FOR NORTH CAPE AXIS, SEGMENT II GIVES PRESSURE CENTERS ASSOCIATED WITH MOVEMENT OF MX ALONG ABOVE AXIS

• Cyclonic center   O Anticyclonic center   + Partial minimum   ◆ Crest of high pressure
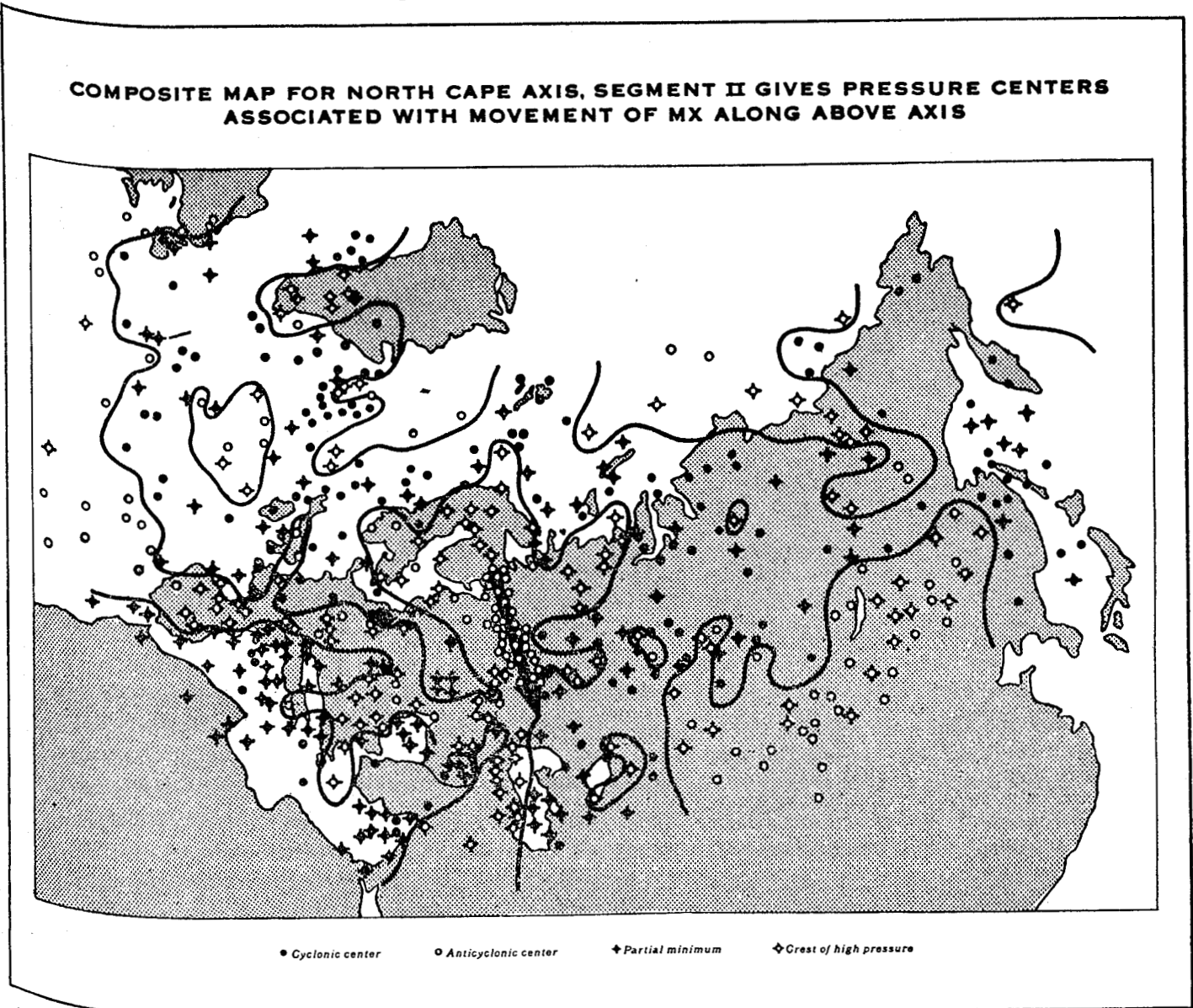
FIGURE 1.—Reproduced from Asknazy (6).

lation of low or high pressure are called fields (cyclonic, anticyclonic, etc.).

It was found that when the process is oriented in a definite manner as, for example, when a development takes place along a certain axis, or when a certain weather phenomenon occurs in a definite region, and in other analogous cases, the centers are grouped into definitely distributed fields of high and low pressure. Only when this condition is observed can one figure on getting well defined pressure fields. In turn, when this selection is carefully observed, *the change which appears in the pressure*

practice. Thus, according to Multanovski, the pressure fields and axes are closely connected with each other, and with the aid of the axes one can determine the distribution of pressure fields and conversely a change in the orientation of the process can be determined from the distribution of pressure fields. This contention was shown by Asknazy to be true only in a very limited degree.

The orientation of a process or phenomenon corresponding to a certain pressure distribution associated with it is established in part by entering on a map the successive positions of centers of high and low pressure.

The composite maps are illustrated with the corresponding temperature, precipitation and wind distributions associated with a given process or phenomenon, so that the weather can be represented by them in detail. The conclusion was drawn that a weather type can best be identified by a combined composite map, kinematic and statistical, which is a map on which we trace the movement of anticyclones and on which are also entered the aggregate pressure distributions (centers of maxima, minima, ridges of high pressure, secondary minima, troughs of low pressure) associated with the passage of the anticyclone in question. The trajectory of the anticyclone allows a determination, as a first approximation, of the origin and extent of modification of the air mass; further details of the weather are obtained from the pressure distributions.

The time interval which characterizes the weather type and during which the pressure centers continue to be situated in separate, closed-in regions forming definitely marked pressure fields is called the natural period time interval. This is the statistical criterion of a natural period. Kinematically expressed, there is no intersection of trajectories during such a period. A change in pressure sign within the confines of the pressure fields, i. e., when the appearance in the region occupied by centers of low pressure of areas of high pressure, and vice versa might serve as an indication of a change of axes, and hence of the arrival or beginning of a new period.

Multanovski's investigations showed that an "operation" along any definite axis continues for 10 to 12 days, the so-called "natural" synoptic period. Hence 2 to 3 days after the beginning of the new period, as soon as the orientation of the process becomes apparent, it is possible to determine the weather for the next 7 to 10 days. The orientation of the process becomes apparent from the initial direction of the trajectory of the anticyclone and from the composite map of the pressure distributions associated with the anticyclone during the time interval marking its initial stage of travel. Because of the close connection which exists between the axis of an anticyclone and the pressure distributions associated with it the subsequent development of the synoptic map and hence the actual weather type can be determined. For if we find that a composite map covering a certain period of time is very similar to the composite map for a given process or phenomenon, we can then say that this map contains the basic features of the indicated phenomenon. If the phenomenon is peculiar to a definite place or region then, without doubt, in many cases it will not attain its fullest development, though it will occupy, with its pressure distribution, a definite area.

Multanovski's experience seems to have been that certain phenomena or processes in U. S. S. R. materialize within a period not nexceeding 30 to 35 days after the appearance of certain significant developments. In other words the orientation of a process can in some cases be established earlier. In general it was concluded that indications of future pressure distributions appear on preceding maps and that we can get an idea of the presence of a maximum number of such indications by the construction of composite maps.

Following through the development of a large atmospheric disturbance in U. S. S. R. one can select a number of phases which precede the occurrence of such a disturbance and also some of the stages which lead to the reestablishment of the disturbed equilibrium. This should be true also of extended processes. It was observed that in the occurrence of any typical phenomenon, preceded by a whole series of changes leading to the phenomenon, there is often observed 30 to 35 days previously, some change, a sort of "flaring up" of a new process. Within this indicated time interval one can distinguish approximately five more "moments." The pressure distributions corresponding to these "moments" represent the initial steps in the phases of the development of a certain process which culminates in the investigated weather phenomenon or leads to a development along an axis having a definite direction.

The determination of phases for various phenomena is made difficult to a large extent by reason of the fact that once a phenomenon characterizes a given season, that is, conditions during a certain season are favorable for the realization of this phenomenon, then the latter has the tendency to repeat itself more than once. What ordinarily happens is that during the development of the process there appears a new threat of the same event, and hence, there is noted a new series. Thus, during a season, a given event may repeat itself six to eight times. Naturally here too there exists a definite regularity. Nevertheless such combinations make the investigations much more difficult. In the future, investigations of this sort will become materially facilitated by the circumstance that in the realization of phases a part must be played by coupled centers of action and definite natural combinations of axes. At the present it is already clear that there cannot be a large number of phases, even if there is a 30 to 35 day interval between the "threats" and the event itself, because the changes in the centers of action cannot proceed at a very fast rate. The length of a phase ranges from 6 to 15 days. On the average there are four to five phases to a period of 30 to 35 days.

3. FORECASTING FOR SEASONS

One of the fundamental principles of forecasting for a period is that, knowing the direction of the segments of trajectories of the significant pressure points during the period, one can, in principle, give the weather characteristics of this period.

It was noted that the composite maps for a period represent a rather simple distribution of pressure formation and in this respect they closely resemble maps of phases and separate weather phenomena.

However, a seasonal composite map constructed in a similar fashion would not, because of a large number of centers of opposite sign lying close to each other, lend itself to a simple division of fields and hence a simple characterization of the distribution of pressure centers. Accordingly the ordinary centers are joined, usually 3 to 4 centers, of the same sign, which lie near one another and which are part of a natural time interval. The seasonal maps appear to be much more complicated since on them are reflected the developments along the entire combination of seasonal axes, the occurring weather phenomena characterizing the season as well as the first signs of new operations characteristic of the coming season. On account of this the pressure fields occupy a much smaller area on seasonal maps and are distributed closer to each other than on the maps of the periods. If the centers of maxima and minima were to be carried over directly from the composite maps for the periods onto the seasonal maps, pressure fields of both signs would no doubt also have been obtained but the dividing line or zone would also have been filled with signs of both kinds, making the analysis of such a zone considerably more difficult. A season consists of natural periods which are characterized by definite operations. In order to divide the

year or season into periods it is necessary to enter on a map, beginning with a certain date, the centers of pressure formations up to the time the pressure fields change their signs. From the moment when, for example, an anticyclonic symbol appears on the cyclonic field, or the reverse, the period shall be regarded as finished, and a new one is at the same time begun. Having established the end of a period and obtained a composite map thereof, the latter is completed in the same manner by entering the preceding days of the period and thus its initial day is obtained. In practice such a division offers a convenience in that given a pair of initial days, one can indicate the entire distribution of pressure areas which will hold till the end of the period.

The season itself is determined by the predominance of a certain axis which serves as its dynamic characteristic and distinguishes it from other seasons. One can say that the beginning of each season is determined on the basis of some completely developed characteristic seasonal process which later may repeat itself several times during the period in question. A curious property of the season appears to be a constancy of certain areas of maxima and minima for the entire interval corresponding to it on all the composite maps. This allows a determination of the weather types.

In preparation of forecasts for seasons the procedure similar to that used in shorter periods is followed. Given composite maps for a set of adjacent seasons the state of pressure fields of the season just ended is compared with the one which preceded it. First are noted regions where the pressure has assumed the reverse sign, an indication of the beginning of one season and the ending of another; then the regions where the pressure sign has not suffered any changes are located and finally the regions where the fields have experienced smaller changes, for example where maxima become supplemented with ridges of high pressure and minima with partial minima, or the reverse, are determined. In this way a map of tendencies is obtained. Also considered are the displacements of pressure fields within the preceding season itself.

Denoting all the customarily encountered changes in the state of pressure with special symbols and applying the latter to the case under consideration a geographical distribution of the changes in the pressure field as well as the character of these changes is obtained. Such a comparison enables one to establish in which direction the change in pressure conditions takes place, and thus, by extrapolating, the composite map of the coming season can be constructed.

Multanovski also states that the extrapolation is made markedly easier and endowed with a larger probability, especially for the districts where a change in sign takes place, if both halves of the just ended season are worked up in an analogous manner.[2] This would lead one to presume that there are two main periods to a season. By examining the distribution of pressure areas on the seasonal map one can select from the atlas of composite maps the expected location of the typical axis. By making use of temperature, wind, and other characteristics of typical axes, one can obtain a whole set of details which have practical interest. Analogously, from the similarity of the pressure distribution on the composite map during specific and seasonal situations one can determine the weather types, characteristic of the season and districts where they occur.

As a result of the comparison the extrapolated map is

[2] These halves are determined on the composite map through the circumstance that there occurs at about that time a sort of attempted shift of the pressure fields, an inception of a new process.

usually found to contain two or three separate types of weather. In order to bring about the division of the composite map into natural periods it is necessary to establish whether all the pressure regions which appear on the map can exist simultaneously, and if not, to select those situations which are common to one another and are coexistent.

Although it is known that there usually exist two or three types it is not possible yet, unfortunately, to establish the order of sequence of the types. Therefore, in making the forecast for a season it can only be indicated that the season will be characterized by the presence of such and such weather types without indicating the exact time of their realization. This is obtained later when forecasts for natural periods of time are issued.

An attempt is also made to establish the time interval during which a particular development takes place for those cases where a similarity exists between the composite map of a season and that of a phenomenon. Knowing that during a certain season such and such a phenomenon may occur it is often possible to find the sequence of operations along different axes over a period of nearly a month. Another means of determining the sequence of seasonal processes is the method of analogous cases. It is pointed out however that there are very few strictly analogous cases so that the application of this principle is limited. This method of analogous cases has been found to be most useful in case of air masses arriving in the U. S. S. R. from the northeast.

4. CLASSIFICATION OF METHODS

The methods used in preparation of forecasts can be classified as follows:

1. *Method of analogous cases.*—Two or more statistical composite maps can be regarded as analogous if when properly constructed they should prove to be kinematically analogous. This method is used but seldom.

2. Extrapolation of dominating processes from one composite map to another which gives the trajectories and positions of regions of maxima and minima. This is made on the basis that the process under investigation is often conditioned by a set of long lasting, almost constantly directed, currents in the atmosphere.

3. *The application of phases.*—For, as was found, a synoptic period appears very often as a phase of one or several processes which will occur in the near future. The phase method is considered one of the best. For, knowing the structure of the composite map and consequently the nature of the processes corresponding to the following phases, one can often indicate, quite accurately, the general character of barometric processes for the following period and sometimes also for the period following it.

4. A known set of empirically obtained relationships between processes of two consecutive seasons.

A systematic verification of the forecasts was made in 1931 by G. Wangenhein who examined 178 long-range forecasts issued during 1928–30. It appeared that the forecasts covering natural periods of time gave an 89 percent verification for precipitation, 75 percent for temperature and 67 percent for the combination of the two. He also found that "forced" forecasts, i. e., not for natural time intervals gave a much lower verification. Since 178 forecasts were issued over a period of 3 years it would appear that Wangenhein verified the relatively short-range forecasts. The seasonal forecasts were verified by Asknazy indirectly, by comparing the forecasted number of pressure centers of either sign with the number obtainable from chance distribution. The highest value

which Asknazy obtained was for the winter season, 56.5 percent.

5. CONCLUSIONS

Asknazy's critical study shows that many of the basic principles which underlie Multanovski's method of forecasting, at least for periods of over 2 weeks, are either faulty or are capable of providing but a very weak foundation for the forecasts. As examples of these principles may be cited the crude definition of weather types by means of composite maps, the absence of an orderly sequence in the movement of anticyclones along the various axes, the indefiniteness of phases in the development of a particular synoptic process and the indefiniteness of the indications preceding it. On the other hand, the recognition of a natural time interval in the development of synoptic processes, the consideration of analogous cases, the use of a recorded and carefully classified weather history,[3] all this coupled with the extensive experience gained by Multanovski in the course of many years would indicate that somewhat better than chance forecasts could be obtained. The independent and indirect verification by Asknazy of the composite maps on which seasonal forecasts were based (and not of the forecasts themselves) shows this to be true only in a very limited degree. It would appear from Wangenheim's study that for shorter periods up to 2 weeks the forecasts are considerably better.

[3] These factors also play an important role in Baur's method of forecasting, described elsewhere in this volume.

GENERAL REMARKS ON MULTANOVSKI'S METHOD OF FORECASTING

By H. C. WILLETT

The methods of the Russian school of long-range weather forecasting, as developed by Multanovski and his associates and the essential features of which are outlined above, are open to a number of criticisms. Of course, it must be remembered that synoptic methods of weather forecasting inevitably must be shaped more or less specifically for the region in which they are to be applied. Consequently one must always be careful in criticising such methods as applied to one region on the basis of experience in another region. Nevertheless, in the light of general synoptic practice and experience, the following remarks on Multanovski's methods would seem to be much to the point:

(1) The lack of any clear definition of the prevailing weather types or synoptic conditions by the composite maps. It is obvious from the irregularity of atmospheric phenomena that the composite map must become more complex, so that the regions in which only high or only low pressure centers shall have occurred must become continually smaller the longer the period which is covered. This appears in the recognition by Multanovski that seasonal composite maps are much more complex than those covering only one "natural" period. But even a simple composite map, one which covers a comparatively short period in which the distribution and movement of the various pressure centers changes but little, can give only an inadequate picture of the distribution of the meteorological elements. We can get widely varying meteorological conditions over extensive regions with almost the same distribution of surface pressure centers. Especially is the distribution of rainfall difficult to determine with any accuracy from a consideration only of location and movement of the various pressure centers at the ground.

(2) The difficulty of distinguishing clearly between weather types, or of terminating definitely the natural period characterized by one weather type. This whole concept of definite weather types and corresponding periods is one which is quite subjective and arbitrary, especially when only surface pressure formations, and the lesser migratory as well as the larger semipermanent centers of action, are included in the picture. It may be possible to characterize to some degree large scale weather types on the basis of location and intensity of the principal centers of action, but it is impracticable to try to refine this sort of representation to the extent which Multanovski does.

(3) The occurrence of preliminary "phase" characteristics preceding the appearance of specific meteorological phenomena in definite regions. The most impractical of any of the features of Multanovski's forecast methods is doubtless that by which he claims to be able to identify characteristic formations in the pressure field, in certain regions, features which make their appearance from 30 to 35 days before the development of certain important weather phenomena in other regions, and which normally repeat themselves four or five times in the meantime. This principle is so contrary to all synoptic experience that it can only be explained as a subjective phenomenon dependent entirely on favorable individual interpretation of the preceding and subsequent events. The

(4) Indefiniteness of forecasts and verifications. A few actual forecasts, for periods from a few days to a few months ahead, depend upon the persistence or the termination and change of the weather type in each region, on the extrapolation of the movement of the areas in which a given type prevails, and upon those phase characteristics which are supposed to presage the development of certain phenomena in definite regions. It is scarcely to be expected that forecasts prepared on such a basis could be very successful. Yet for 178 forecasts issued between 1928 and 1930 there is claimed a verification of 89 percent for precipitation and 75 percent temperature forecasts. The reason for the high verification is to be found in the wide limits included within the temperature forecasts, and the latitude which is allowed in the verification of precipitation. It is certain that a rigid check of these forecasts would give appreciably poorer results than those quoted above.

BIBLIOGRAPHY

[The publications dealing with the theory and methods of the long-range forecasts discussed above are, with one exception, in Russian. Most of them are accompanied by brief résumés in English, French, or German. The two papers in which an exposition of the methods is attempted or made in part are listed as numbers (4) and (5). No paper contains a complete exposition of the subject. The order of the papers listed below is historical]

(1) B. P. Multanovski. The influence of Centers of Action of the atmosphere on the weather in European Russia during the warm season. Geophys. Collect. v. 2, No. 3, pp. 73–97, 1915.

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(3) B. P. Multanovski. Northeast storms on the Black sea and their significance for the synoptic situation of Europe. Bull. of the Centr. Hydromet. Bur., No. 3, pp. 45–54, 1924.

(4) E. I. Tichomirov. Ten years of regular long-range weather forecasts. Bull. of the Centr. Geophys. Obs. No. 3–4, pp. 3–10, 1931.

(5) B. P. Multanovski. The present (1933) status of development of long-range weather forecasting methods in U. S. S. R. Met. Messenger, v. 43, No. 5–7, pp. 129–143, 1933.

(6) A. E. Asknazy. On the question of long-range weather forecasting methods. (An attempt at an analysis of B. P. Multanovski's method.) Met. and Hydr. No. 10 (1936), pp. 3–40, No. 11 (1936) pp. 3–43.

INTRODUCTION TO REPORTS ON VARIATIONS OF THE SOLAR CONSTANT AS A FACTOR IN LONG-RANGE WEATHER FORECASTING

By LARRY F. PAGE

A short historical summary of the work of the Smithsonian Astrophysical Observatory relating to long-range weather forecasting may be of interest in connection with the following reports on its investigations of the solar constant. A complete account may be found in the several volumes of the Annals of the Astrophysical Observatory, published by the Smithsonian Institution.

The first observatory was constructed in 1890. Its possible ultimate usefulness toward long-range weather forecasting was suggested as early as 1892 when Langley wrote, in the report of the Secretary of the Smithsonian Institution:

We are sure that the knowledge (of how the sun affects the earth) would form a scientific basis for meteorology and enable us to predict the years of good or bad harvests, so far as these depend on natural causes, and yet we are still very far from being able to make such a prediction. * * *

Attempts to measure the total radiation of the sun were begun about 1900 in Washington, D. C., and a few years later at Mount Wilson, in California. Probably the first attempt to correlate variations in the solar constant with those in the weather was published by Langley in the Astrophysical Journal for June 1904. Variations of as much as 15 percent had been found in the solar radiation, which seemed to Langley and Abbot to be much greater than possible errors of instruments or methods. Later evidence, however, shows that no real variation of this magnitude exists.

Between 1910 and 1920 several investigators, including Clayton, Arctowski, Helland-Hansen and Nansen studied the relation between measurements of the solar constant and various weather factors. Although Clayton found that these relations were different for different stations and seasons, he was led to believe that "the fluctuations

(of temperature at Buenos Aires) from mean conditions * * * may depend principally upon the variability of the sun."

The first weather forecasts directly associated with the work of the Astrophysical Observatory were made experimentally by Clayton beginning in the fall of 1923, on the basis of solar-constant measurements. Forecasts were made 3, 4, 5, and 27 days in advance, but only the first three indicated any real foreknowledge of the weather. More general weekly and monthly forecasts were later made. This phase was discontinued as part of the Smithsonian program at the end of 1925.

Especially during the late twenties, studies were made of periodicities in the solar constant. These were used for forecasting the subsequent variation of the solar constant, but not publicly, at least, for weather forecasting, although similar periodicities were claimed in weather data. Further connection between solar radiation and weather was studied in the "patterns" of weather following certain solar changes. These two phases of the work are discussed in the third part of this report.[1]

Throughout the years since 1900 increasing accuracy has been attained in measurements of the solar constant. Before the Astrophysical Observatory entered the field it was known only that the mean radiation probably lay between 1.5 and 4.0 calories per square centimeter per minute. The most recent published results limit the range to 1.93 to 1.95, a notable achievement of ingenuity, resourcefulness and patience against great odds. The first two parts of the following report deal with the accuracy of these measurements.

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[1] Some of the conclusions may be compared with those obtained by M. M. Paranjpe (Q. J. R. M. S., 64:459–476 (1938)), in an entirely independent investigation. (Reply to the above by Abbot was published in Q. J. R. M. S., 65:215–236 (1939).

ACCURACY OF SMITHSONIAN INSTITUTION SOLAR CONSTANT MEASUREMENTS

By Herbert G. MacPherson

CONTENTS

I. METHOD OF DERIVATION OF SOLAR CONSTANT

The solar constant is the energy from the sun that would reach a square centimeter of area normal to it at the earth's mean distance from it in each minute if there were no atmosphere. It is a measure of the rate at which heat is given off by the sun. The amount of heat received by a small area can be measured directly by means of a pyrheliometer, but as the measurements are necessarily taken through a portion of the atmosphere, they do not represent the whole amount of heat given off by the sun that reaches the outside of the atmosphere. The correction for loss of heat by absorption and scattering in the atmosphere presents the great problem in deriving a solar constant. There are two methods used for making this correction, the "long" and the "short" methods.

A. THE LONG METHOD

The long method requires observations of the solar intensity of each wave length of radiation at several effective thicknesses of atmosphere. The intensity of each wave length outside the atmosphere is extrapolated from these observations. The method takes about 2½ hours, and assumes that the atmosphere remains uniform over this period of time. The long method requires two observing instruments, the pyrheliometer and the spectrobolometer. The pyrheliometer measures the total radiation reaching the instrument, and the spectrobolometer determines the relative energy in each wave length.

B. THE SHORT METHOD

In the short method the atmospheric depletion of the solar radiation of each wave length is estimated from a determination of the brightness of the sky near the sun, and from the amount of water vapor in the air. The method is secondary in that the determination of atmospheric opacity is not absolute, but depends upon a comparison with the average results of a large number of days

of long-method observation. The short-method observations can be completed in 10 minutes, and so are relatively independent of atmospheric changes.

The short method requires three observing instruments. The pyrheliometer is used as before to get the total intensity of radiation at the surface of the earth. The amount of water vapor is determined from the area of a water-vapor absorption band as determined by the spectrobolometer. The brightness of the sky near the sun is measured by use of a pyranometer.

II. INSTRUMENTAL AND OBSERVATIONAL ERRORS

An attempt will be made to give an account of the possible sources of error in the instruments and in reading them, and to give an estimate of the magnitude of these errors. Such estimates are not expected to be as satisfactory as estimates based on comparative readings, but they will be useful in showing what order of magnitude of errors is to be reasonably expected. Only relative errors will be considered, as we are interested only in changes of the solar constant.

A. PYRHELIOMETER

The pyrheliometer consists essentially of a small blackened silver disk in contact with a mercury thermometer. The disk is exposed to the sun's rays for a short time and the rate of rise of its temperature is determined by reading the thermometer at definite time intervals. The rate of rise of temperature is proportional to the rate at which the surface receives heat. The disk is sufficiently insulated so that it receives appreciable heat only from the radiation of the sun and the sky immediately surrounding it. It is fortunate that this is the fundamental instrument used, for its simplicity makes it possible to evaluate almost all possible errors.

1. *Cooling correction.*—The method of observing is as follows: The outside cover is removed from the instrument. Twenty seconds later the temperature T_1 is read. After 100 seconds it is read again, as T_2. The shutter is then opened, exposing the disk to the sun's rays. After 20 seconds the temperature T_3 is read and 100 seconds later the temperature T_4. The shutter is now closed and after 20 seconds T_5 is read. One hundred seconds later T_6 is read. The temperature differences $T_5 - T_6$ and $T_2 - T_1$ give a measure of the rate at which the disk loses heat before and after the exposure to the sun. Their average is taken as the average rate of loss of heat of the disk during the time it was exposed to the sun. Thus the corrected temperature rise for 100 seconds is $(T_4 - T_3) + \dfrac{(T_5 - T_6) + (T_1 - T_2)}{2}$. The question arises as to how valid this temperature correction, $\dfrac{(T_5 - T_6) + (T_1 - T_2)}{2}$, is.

A rough plot of the temperature of the silver disk as a function of time during an observation is given in figure 1. The loss of heat of the disk by radiation will be nearly linearly related to its temperature so long as its temperature does not differ materially from that of its surroundings. The best cooling correction to apply, then, would be one corresponding to its rate of cooling when it is at the temperature midway between T_3 and T_4, that is, at $T_a = \frac{T_3 + T_4}{2}$. The cooling correction used corresponds to some lower temperature T_b, determined by $T_b = \frac{T_1 + T_2 + T_5 + T_6}{4}$. That this temperature is lower is obvious from the graph, figure 1, drawn from real data (vol. 3, p. 51).[1]

Abbot has made a study of the effect of this, and states (vol. 2, p. 73) that every result would be increased by almost exactly 1 percent if the correct method of allowing for cooling were used. As relative values only are wanted, the more convenient cooling correction is used. According to the results of Abbot's tests, then, no relative error is introduced here.

2. *Temperature correction.*—The heat capacity of the silver disk changes with temperature, so that the same amount of heat applied to the disk will produce differing temperature rises depending on its initial temperature. The area of the diaphragm that limits the cross section of the beam hitting the disk necessarily varies in size with temperature, due to thermal expansion. The bore of the thermometer stem will also vary with temperature, as will the radiation emitted by the disk through the opening to the sky. The combination of these temperature effects makes it necessary to apply a correction for the temperature of the instrument. The correction is given (in vol. 3, p. 51) as 0.0011 $(T-30°)$ R. This is a correction of 0.11 percent per degree centigrade, and is determined by comparison of instruments of different temperatures. It seems to me that this figure must be more or less inexact and certainly can not be exact beyond the given significant figures. Therefore we can assume a probable error of at least 0.005 percent per degree centigrade due to this, an error which is systematic with the temperature. A yearly temperature variation of 20° C. would produce a probable variation of only 0.1 percent due to the uncertainty of this correction.

3. *Uniformity of calibration of thermometer scale.*—The thermometers used are marked with equal divisions to 0.1° C. on the stem and then calibrated by comparison with a standard. The absolute values are not important, but the uniformity of calibration is. We can assume that the thermometers are calibrated all right.

4. *Surface of silver disk.*—There is little about a silver disk pyrheliometer to cause it to change its reading with time. The essential constants of an instrument that determine its reading are: The size of the diaphragm admitting the beam of sunlight, the nature of the surface of the disk, the mass of the disk, the thermal connection between the disk and the thermometer, and the scale of temperature of the thermometer. Barring major accidents, all of the above elements will remain constant in time except perhaps the nature of the surface of the silver disk. It should be nearly perfectly absorbing and remain so.

[1] References made omitting the name of the publication are to Annals of the Astrophysical Observatory, Smithsonian Institution. Dates are as follows: Vol. 1, 1900; vol. 2, 1908; vol. 3, 1913; vol. 4, 1922; vol. 5, 1932.

The surfaces are prepared by painting with lampblack suspended in shellac. References made to occasions when a complete cleaning and reblackening was tried are as follows: Volume 2, page 76: Pyrheliometer IV, after 6 months occasional use, was cleaned (of dust) and resmoked, and read 0.7 percent higher. Volume 3, page 51: A copper disk several times cleaned and reblackened, "shows no evidence of changes as great as 1 percent." Volume 5, page 134: Reinsertion of thermometer and reblackening of silver disk gave 0.92 percent lower reading to S. I. 17. If it were not for dust, a surface once put on probably would not change much, say less than 0.1 percent. But as Abbot says, volume 2, page 76: "Despite all care which can be taken, dust collects on the smoked surfaces and in time diminishes their absorbing power."
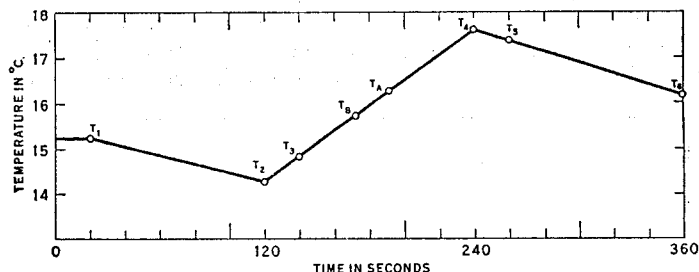

FIGURE 1.—Approximate variation of temperature of silver disk with time.

References to the effects of removal of dust are numerous. Volume 2, page 76: Pyrheliometer II, having been unfortunately left open in the laboratory in the winter months, was freed from dust by vigorous blowing (September 9, 1906), and found to read 1.4 percent higher. At Mount Montezuma (vol. 5, p. 86), the pyrheliometers in daily use were discovered to be dusty on April 24, 1924. The dust was removed and a 2.8 percent rise in readings was noted. After this, observers were warned to be careful to close the shutters of their instruments and to brush off the silver disks with fine camel's hair brushes occasionally, and to make frequent comparisons with the standards held in reserve. "Whenever appreciable corrections for dust seemed indicated, which fortunately was rarely, they were made as accurately as possible."

At Table Mountain (vol. 5, p. 139) dust was removed from S. I. 32 after May 31, 1927, giving about a 1.9 percent rise. Removal of dust from A. P. O. 10 gave about a 0.5 percent rise in readings. These were dusted frequently since then.

It is probable that except for the noted dust corrections, the changes due to dust are small. Frequent comparisons with carefully preserved standards indicate this. Thus, table 20, volume 5, page 134, shows the comparisons of the standard with one of the pyrheliometers in daily use at Montezuma from 1919 to 1930. (The years when dust accumulated, December 1922 to March 1924, had no comparisons, as the director, Aldrich, did not know of the presence of the standard instrument.) The standard deviation of 32 average ratios of the regularly used instrument to the standard is only 0.214 percent, indicating that large changes did not ordinarily take place.

5. *Effect of region of sky observed.*—The pyrheliometer has a "vestibule" which limits the area of the sky observed by the instrument. The beam of sunlight is limited to an area less than that of the silver disk, but each point of the silver disk can "see" or receive light from a region of the sky about the sun. Figure 2 illustrates this.

Thus the pyrheliometer reads high by the amount of radiation that the sky scatters in its direction. If the scattering power, or haziness of the atmosphere were constant, the readings of the pyrheliometer would still be proportional to intensity of the sun for a given zenith distance of the sun. But the haziness varies between wide limits, and readings of the pyrheliometer on days of different haziness are not comparable if the scattered radiation from the sky is appreciable.

Abbot has several times tried to show that the effect of the sky light was negligible. In volume 3, page 51, he says: "As to the effect of variations of the light of the sky, it might seem that since the pyrheliometer is exposed to 80° of solid angle, of which the sun occupies only about 0.2°, the sky light might be quite considerable. To test this question a screen which limited the solid angle to 5° was fixed to one instrument, and another instrument with the usual arrangements was compared with it at Washington. No alteration of the relative readings due
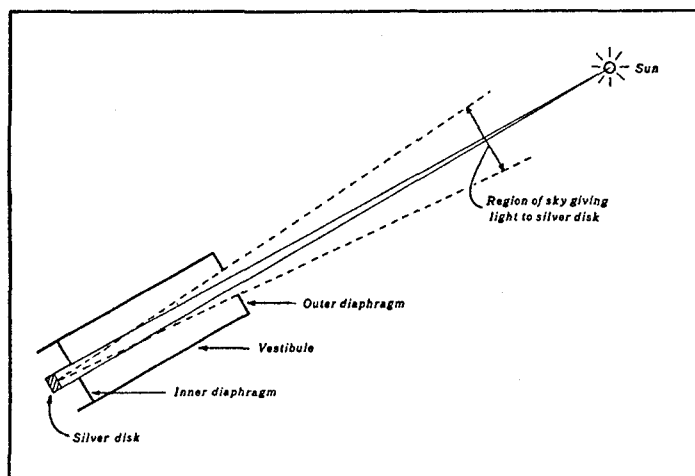

FIGURE 2.—Region of sky observed by pyrheliometer.

to the use of the screen could be found on a very clear day. On another day, less clear, a change of readings of about 0.5 percent was found. On a very poor day the effect may reach 1 or even 2 percent. On Mount Wilson the sky is so clear that its effect would be negligible."

Referring to these tests in volume 5, page 82, he says: "At the time these tests were made we did not expect to observe the solar constant on such hazy days as those sometimes employed at Harqua Hala, nor did we regard errors of 0.5 percent as important, so that the results seemed to be fully reassuring that the sky error is negligible."

New tests were made. At Harqua Hala two instruments were compared, one with the usual 0.0046 hemisphere of the sky exposed and the other with a special vestibule of 0.00027 hemisphere. The day chosen, February 4, 1925, was noted as "extremely hazy, so much so as to be exceptional." From the comparison it was shown that the reduction of 0.0046 to 0.00027 hemispheres diminished the reading 2.44 percent.

At Montezuma, on a "very hazy day," March 24, 1925, a comparison was made between two pyrheliometers, both when one of them had the 0.0046 hemisphere aperture and when it had only a 0.00054 hemisphere aperture. A difference of only 0.2 percent was noted. I have calculated the probable error of this difference to be 0.14 per-

cent, assuming the probable error of a single pyrheliometer reading is 0.3 percent. Abbot says: "It is indeed possible that a small sky error, amounting to a few tenths of 1 percent, may affect Montezuma pyrheliometry on very hazy days. * * * The system of corrections which the statistical studies had fixed must tend to eliminate the errors, if any, due to sky effect."

The vestibules of the pyrheliometers at Montezuma were unchanged, while those at Table Mountain were changed to give an effective sky area of 0.0004 hemispheres. The new pyrheliometers (1930) expose 0.0013 hemispheres.

In volume 5, page 84, Abbot attempts to show from some results of Ångström that the sky effect is negligible. Ångström measured the total sky radiation to and from a blackened strip at noon and before and after sunrise. The blackened strip, being a perfect radiator, radiates its energy to the sky at all times, depending only on its temperature. The sky radiates back energy corresponding to its temperature, but being effectively colder than the strip, it radiates much less, so that there is a net loss of heat of the strip at night. During the day scattered sunlight also tends to heat the strip, and at noon a slight gain of heat of the strip was noticed, amounting to 0.07 calories per square centimeter per minute. (The direct sun's rays were shaded.) Abbot takes this difference of scattered solar radiation minus radiation by the strip to calculate the sky effect. The sky effect so calculated comes out to be 0.005 calories per square centimeter per minute or 0.25 percent in an ordinary pyrheliometer. This procedure is entirely erroneous, as the radiation of the silver disk depends only on its temperature, and is entirely independent of the nature of the sky to which it radiates. Furthermore, its variation of radiation with temperature is already taken care of in the temperature correction of the pyrheliometer. Taking the difference between the net radiation of the sky at night and at noon as measured by Ångström, which is 0.27 calories per square centimeter per minute, the same calculation reveals the sky effect to be 0.02 calories per square centimeter per minute or 1 percent of the solar constant. This estimate is a little low because the region about the sun, which scatters most light, is shielded by the sun shade. However this may be compensated by the fact that the observations of Ångström were taken on a "very hazy day" at an altitude of only 1,300 meters on Mount Bassour in Algeria.

Dorno (Monthly Weather Review 53, 519 (1925) and vol. 5, p. 84) shows from photometric measurements that on clear days at high altitudes the apparent increase of solar brightness by visible sky rays entering the old form of silver disk pyrheliometer (used at Montezuma at least till 1930) is about 1¾ percent. He states that, for total radiation of all wave lengths, the effect would be considerably less, but not negligible.

Brazier, Masek, and Guilhen (Comptes Rendus 199, 644 (1934)) compare two pyrheliometers of different aperture and find that there is a systematic difference between their readings, depending on the atmospheric transmission coefficient, which is a measure of the sky scattering (i. e., the less transparent the sky, the greater is the scattering of light by the sky.) The ratio of the readings of the two pyrheliometers was found to be 0.822+0.13p where p is the atmospheric transmission coefficient. Their results indicate that there is a sky effect.

It is possible to calculate from some of Abbot's own work what the magnitude of the sky effect will be. In volume 3, page 145, Abbot gives an account of measurements of the sky brightness near the sun, made on Mount Whitney on a day of "sky of highest excellence." Copying part of his table 51, page 147, we have:

The last line gives the ratio of the effect of each zone to the total solar intensity, and is a measure of the "sky effect" for that zone. These are plotted in the lower curve on figure 3 and extrapolated to the edge of the sun.

The extrapolation gives 0.58 percent in the zone up to 1.5° from the sun. Adding this to the intensity from
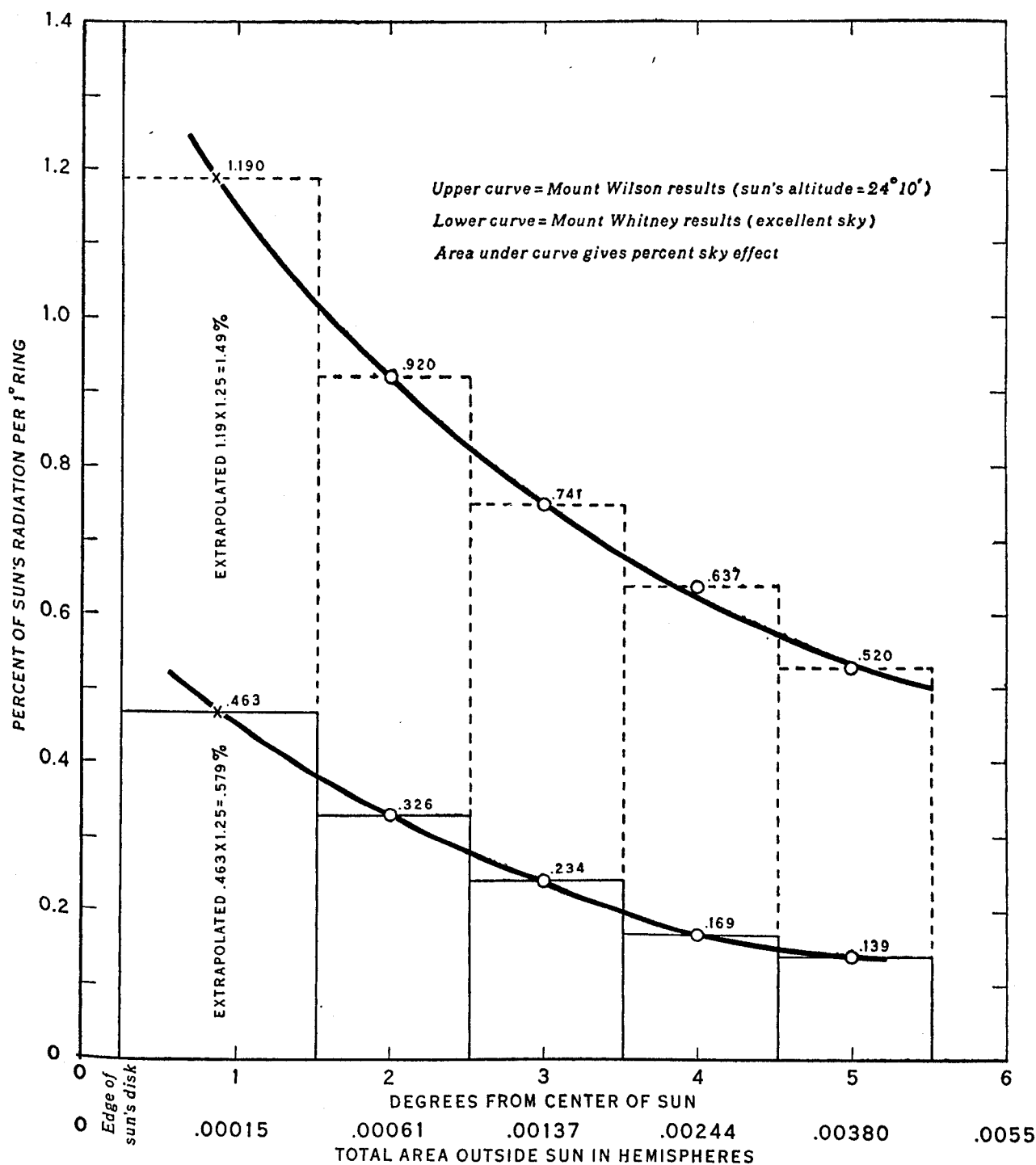
FIGURE 3.—Extrapolation of sky effect to solar disk.

Solar distance (degrees)	2	3	4	5	6
Brightness of sky expressed as a fraction of the average sun's brightness ×10⁴	5.68	2.7	1.48	0.966	0.744
Ring of solar distance (degrees)	1.5–2.5	2.5–3.5	3.5–4.5	4.5–5.5	5.5–7
Area of ring in hemispheres	0.00061	0.00092	0.00121	0.00152	0.00285
Brightness × area × 10⁴	3.46	2.48	1.79	1.47	2.12
Last line divided by area of sun (=1.06 ×10⁻³)	.00326	.00234	.00169	.00139	.002

the other zones, we get 0.579+0.326+0.234+0.169+0.139 =1.4 percent, as the sky effect for a pyrheliometer which views 0.0046 hemispheres, as those used at Calama do. When the tests of sky effect mentioned above were made at Calama, the difference between two vestibules of 0.0046 and 0.00054 hemisphere exposure was found to be 0.2 percent±0.14 (probable error). At an aperture of

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0.00054 hemisphere the results at Mount Whitney indicate that a sky effect of about 0.7 percent, or one-half of the total sky effect at 0.0046 hemispheres aperture, still exists. Thus the experiment at Calama only measured one-half of the total sky effect, and the probable error is sufficiently large that possibility of a sky effect of 1 percent is not unlikely. The results at Mount Whitney should be comparable to those at Montezuma, as Mount Whitney is about 15,000 feet high and Montezuma is 9,000 feet.

Similar tests were made by Abbot at Mount Wilson, reported in the Astrophysical Journal, *28*, 129 (1914). The results are shown in the upper curve of figure 3. They give a sky effect of 4.3 percent for the usual pyrheliometer. These results were taken at a solar altitude of 24° 10′, and the indications are that they would be reduced by a factor of 3 at an altitude of 65° of the sun, that is, to 1.43 percent.

Calculations from Ångström's results, the results of Dorno, and calculations from Abbot's own results all indicate that a sky effect of 1 percent is probable on a hazy day at Montezuma. The vestibules used at Table Mountain exposing 0.0004 hemispheres are seen from figure 3 to have still one-third of the sky effect at 0.0046 hemispheres, while the new pyrheliometer, viewing 0.0013 hemispheres, have two-thirds the sky effect of the old form.

This sky effect that I estimate to be about 1 percent at Montezuma means that at high sun the scattered light from the sky about the sun contributes 1 percent to the total reading of the pyrheliometer. So long as the sky effect is constant, it will not affect the changes of solar constant observed. However, it varies a great deal with changes of atmospheric transparency, increasing as the atmospheric transparency diminishes. This effect provides one means by which derived solar constant values can be related to terrestrial phenomena.

6. *Reflection from parts of vestibule.*—No errors are noted for this in connection with the pyrheliometer, though a big error is noted for the pyranometer from a shiny vestibule, an error of 20 percent. In the case of the pyranometer the direct sun is excluded and the radiation of the sky observed. Any reflection of the relatively much brighter sun is, then, likely to cause a big error in the pyranometer. In the pyrheliometer the relative errors are likely to be much less, and as none are noted, we can probably assume that they are small or absent.

7. *Errors in pointing the instrument.*—At Table Mountain it was found in 1930 (vol. 5, p. 251) that solar constant values were running 0.4 percent lower by tests of selected pyrheliometry. "It was suspected that the pyrheliometers might be pointing unfavorably." One can only guess as to whether slight errors of this kind might have occurred at other times.

8. *Errors in reading the time.*—In taking a pyrheliometric observation, ordinary errors will occur in reading the temperature and in determining the time for each temperature reading. Before 1925, individual watches were used to determine the time for readings. In addition to the natural error due to human limitations in reading the time, the eccentricities of the second hands of the watches used introduce error. Volume 4, page 92: "If two observers make a series of comparisons between two pyrheliometers, and then interchange instruments and make another series, almost 1 percent difference between the two series is sometimes caused by the eccentricity of their watch hands if this is neglected," and "It occurs

that in some watches 0.3 seconds, or 0.3 percent error is introduced by eccentricity."

In volume 5, page 81, Abbot says, "As eccentricities of watch hands are apt to change, and as the determination of this is not altogether accurate or very easy, we introduced a new and better method of observing in 1925." The new method of timing makes use of a half-second pendulum. By means of a mercury contact, the pendulum electrically operates the escapement of an alarm clock. The clock has its gearing altered so that the minute hand revolves every 10 seconds, and rings a bell at each revolution. The readings are taken at the sound of the clock escapement. The actual timing of this arrangement should be very accurate, with only human errors appreciable. I estimate that the probable error in getting the correct time before 1925 to be ±0.2 second, and after 1925 as approximately ±0.1 second. These estimates are necessarily unsatisfactory, but it will be illuminating later to see how big an error in a single pyrheliometric determination is to be expected with errors of this size in the timing.

9. *Errors in reading the temperature.*—The thermometers are marked to the nearest tenth degree in a 65° C. range. The readings are estimated to a hundredth of a degree. The instructions furnished with the instruments say to read hundredths of a degree first, then the degree itself. It seems reasonable to take a probable error of 0.004° as a minimum probable observational error. This would mean that 60 percent of the time the nearest hundredth of a degree is read, and that less than 1 out of 10 readings is off by as much as a hundredth of a degree.

10. *Personal equation.*—It was noted in 1929 (vol. 5, p. 137) that some observers consistently read the pyrheliometers higher than others. Comparisons were made between 14 observers of the period 1920–30 and the "percent personal equation" calculated for each. In the table of "personal equations relative to the mean," we find values from −0.29 percent to +0.13 percent, with an average without regard to sign of 0.107 percent. Abbot says about this personal error, on page 138, volume 5: "After reflection we decided not to apply corrections to eliminate these personal equations. They are all small. Excepting in the cases of Aldrich, Baughman, and Butler, they are hardly appreciable and perhaps not beyond the variability of the personal equations themselves. As far as concerns short period solar variations, the personal equations, even if real, will comparatively seldom change the result, because the general practice has been for one observer to read for many days consecutively. As far as they affect the monthly mean values, they will seldom be of importance because the observers will generally have been exchanged during so long an interval as a month, which tends to reduce the error." These so-called personal errors are probably of very little importance, except as indicating that the assumed probable errors of reading time and temperature are not too small.

11. *Total observational error of a pyrheliometer reading.*—In the following estimate of observational errors, it was assumed that on the average a 3° rise of temperature of the pyrheliometer is noted in 100 seconds, and that a 1° cooling correction is applied, giving a total corrected rise of 4° C. This is a fair assumption.

Time errors

Assuming a 0.1 second probable error in timing, the probable error in the cooling correction due to this is—

$$\frac{0.1}{100} \times 1° \times \frac{\sqrt{2}}{\sqrt{2}} = 0.001° \text{ C.}$$

In the 3° rise it is—

$$\frac{0.1}{100}\times 3°\times\sqrt{2}=\sqrt{2}\times 0.003°$$

Therefore the probable error due to time is—

$$\sqrt{(0.001)^2+2(0.003)^2}=\sqrt{0.000019}=0.00436° \text{ C.}=0.109 \text{ percent of a } 4° \text{ rise.}$$

If we assume a probable error of 0.2 second in determining the time, the error due to this is twice the above, or 0.00872° C.=0.218 percent.

Temperature errors

Probable error in cooling correction is $\dfrac{\sqrt{2}\times 0.004°}{\sqrt{2}}=0.004°$.

Probable error in 3° rise is $0.004°\times\sqrt{2}$.

Therefore the temperature reading error is

$$\sqrt{(0.004)^2+2(0.004)^2}=\sqrt{0.000048}=0.00693°=0.173 \text{ percent of a } 4° \text{corrected rise.}$$

Total errors

Assuming a P. E. of 0.1 second in reading time and of 0.004° in temperature, we have for the probable error of a pyrheliometer reading:

$$\text{P. E.}=\sqrt{0.000048+0.000019}=0.008153° \text{ C.}=0.204 \text{ percent}$$

If a 0.2 second probable error is assumed, this becomes 0.01113° C. or 0.278 percent.

I believe that these figures give the lower limits of error for a good observer. It does not take into account slight errors due to inexactness of the temperature correction or due to slight amounts of dust on the silver disk, or to slight errors in pointing the instrument, if any such occur.

Abbot gives several estimates of the probable error from actual tests. In the instructions for use of a silver disk pyrheliometer, page 52, volume 3, Abbot says: "As regards accidental error of observation, persons of good eyesight and experience in observing appear to read with a probable error, for a single determination, at high sun, not exceeding 0.3 percent. * * * It seems almost incredible that this degree of accuracy should usually be attained, but comparisons of instruments by two observers simultaneously, if made under excellent sky conditions, so indicate."

In volume 4, page 162, Abbot gives a probable error at Montezuma and compares it to Mount Wilson. "From data taken * * * at Montezuma, Chile, in 1921, between silver disk pyrheliometers S. I. 30 and S. I. 29, which are daily employed there, the average deviation of these instruments (also read at high sun 1 minute apart) is 0.34 percent. Whence the probable error of one observation on one silver disk pyrheliometer is approximately $\dfrac{0.84\times 0.34}{\sqrt{2}}$ or 0.20 percent, including the probable error due to variation of the sky in 1 minute." Similar comparison of 2 copper disk pyrheliometers at Mount Wilson was 0.37 percent. It seems to me that this difference is rather big, in view of a statement of Abbot's made in Beiträge zur Geophysik *16*, 344 (1927). He says: "Professor Marvin's diagram shows less scatter at

Calama (in solar constant values) than ever was reached at Mount Wilson, but this improvement was not, so far as I know, because of any superiority of instruments or of observing at Calama. It depended rather on better sky conditions." That the test at Calama was made from selected data, or else that the effect of changing sky is enormous in as short a time as 1 minute, is indicated by using the comparisons of the same 2 pyrheliometers from July 1917 to March 1918, volume 4, page 91 of the Annals. From 20 comparisons Abbot gets a probable error in ratio of 0.13 percent. From this, the probable error of a single determination is $\dfrac{0.13\times\sqrt{20}}{\sqrt{2}}=0.41$ percent, much larger than the 0.20 percent obtained at Calama.

In answer to the criticism of Linke, Abbot gives a complete set of (only) 10 observations on page 136, volume 5. The ratio of readings of pyrheliometers No. 29 and No. 17 was determined by 2 observers, changing instruments after 5 readings. I calculate the standard deviation (from the general mean) of the ratio to be $\sigma=\dfrac{0.528}{1.0185}$ percent. From this the probable error of a single determination is calculated to be P. E. $=\dfrac{0.528}{1.0185}\times 0.6745\times\dfrac{1}{\sqrt{2}}=0.247\pm 0.026$ percent. These readings were taken on April 24, 1928 (after the introduction of the new timing system), and are said to be "usual, not specially selected results."

If the so-called personal equation is eliminated in this series by calculating the dispersion of each set of five readings about its own mean, the probable error is only reduced to 0.23 percent. The personal equation between the two men calculates to be 0.34 ± 0.10 percent.

Table 24, page 140, volume 5, gives a table of 110 ratios of pyrheliometer readings taken in Washington from 1917 to 1930. The average number of comparisons is 8.9 for each ratio, and Abbot gives the average probable error of the ratios as 0.18 percent. This gives for the average probable error of a single pyrheliometer reading

$$\frac{\sqrt{8.9}\times 0.18}{\sqrt{2}}=0.38 \text{ percent. This figure is an average for}$$

many pyrheliometers, and includes slight errors of dust, etc., that might occur from time to time. It should be noted that in almost all of these comparisons, one of the pyrheliometers of the pair was new.

Perhaps the best idea of the actual probable error of a single pyrheliometer measurement as they are made in the field is to be obtained from table 20, page 134, volume 5. This table gives the comparisons made from 1919 to 1930 between one of the regularly used pyrheliometers at Montezuma with the standard instrument. Each ratio of the 2 readings is the result of "about" 10 readings of each instrument. The standard deviation of the ratios is 0.214 percent, whence the probable error of a single

$$\text{pyrheliometer reading is } \frac{0.6745\times 0.214\times\sqrt{10}}{\sqrt{2}}=0.323$$

percent. The same result is obtained if we use only the data from 1926 to 1930, indicating that the change in method of timing did not affect the accuracy much. The various results above are summarized in the following table.

From these results I consider the best estimate of the probable error due to accidental observational errors only of a single reading to be given by No. 6, about 0.25 percent ($\sigma = 0.37$ percent). For a probable error suitable to use for longer intervals of time than a few days, I consider the best estimate to be about 0.32 percent ($\sigma = 0.474$ percent) at Montezuma. From the difference between these two estimates, it seems that the probable error to be expected from variations of dust and pointing from time to time is about 0.2 percent. The above figures are for "high sun and excellent sky conditions." It is inferred that at low sun the errors are perhaps greater.

B. PYRANOMETER

The pyranometer is used to get the intensity of scattered light from the sky, from which estimates are made of the sky transparency. The pyranometer used at Harqua Hala and Table Mountain, and at Montezuma since January 1923, exposed a region of the sky 29° in diameter and hides a section in the middle 7° in diameter to shade off the sun. The light goes through a glass screen and impinges on 2 blackened manganin strips, one 10 times as thick as the other. The temperature difference between the 2 is measured by a platinum-tellurium thermocouple. The radiation heating causes a different temperature rise between the 2 because of their different size. Later the same difference of temperature is produced by heating the 2 strips with identical amounts of heat caused by an electric current. The amount of electrical energy needed to do this, which can be measured, is then the same as the energy of radiation received by the strips.

According to Abbot, it takes 20 percent error in the pyranometer reading to make a 1 percent error in the derived solar constant value. Therefore, although the errors in pyranometry are much greater than those in pyrheliometry, they are relatively less important, and I will not consider them in great detail.

1. *Effect of radiation from strip and glass cover.*—Before the cover is removed exposing the strip to the sky, there is temperature equilibrium inside the instrument, so that all parts are at the same temperature and are giving off as much heat in long wave radiation as they receive. When the cover or shutter is removed, this equilibrium is disturbed, as the exposed sky is at a lower effective temperature than the instrument. Consequently the glass cover receives less radiation than it formerly did, and still giving off the same amount, its temperature is lowered. The glass is opaque to this long wave radiation, so that the radiation balance of the manganin strips is affected only by the cooling of the glass cover. This cooling takes time, so that if the readings are taken rapidly, this disturbance of radiation balance will little affect the result. Abbot (vol. 4, p. 76) estimates the error due to this as 0.6 percent. As this error will be of the same sign and will tend to be constant, it will affect changes of solar constant values very little.

As regards the scattered sunlight which the pyranometer is designed to measure, the glass cover is transparent to most of this short wave radiation, while the manganin strips will absorb it nearly perfectly.

2. *Accidental observational errors.*—In any reading of the pyranometer, there are two readings of the galvanometer and one of the ammeter, each of which can be read to a probable error of about 0.25 percent. As the galvanometer reading enters as a square, the probable observational error is about $\sqrt{4} \times 0.25 = 0.5$ percent. After errors of instruments are added in, the total probable error of a single reading is still probably not greater than 1 percent. This would introduce an error of only 0.05 percent in solar constant values, and is almost negligible.

3. *Polish on vestibule.*—However, an error that was not negligible occurred at Table Mountain from 1927–28 and might have occurred to a lesser extent elsewhere. An error which could not be traced was noted to start about August 1927, and it was not until September 1928 that it was discovered that a shiny vestibule was causing at least most of the error. The shiny vestibule reflected direct sunlight onto the manganin strips. The calculated error reached 22.5 percent. After a discovery of the source of error and repainting of the vestibule, an 8 percent correction was still applied to Table Mountain pyranometer values to give solar constant values comparable to those of Montezuma. As to how much of this reflection error occurs at other times it is impossible to estimate. As noted above, it takes about 20 percent error in pyranometry to give a 1 percent error in solar constant.

C. BOLOMETRY ERRORS

The spectrobolometer is an instrument to measure the sun's spectral distribution of the solar energy. The rays are reflected from a mirror through a slit to a prism, where the beam is dispersed into a spectrum. The light from a particular point in the spectrum passes through a slit to a blackened platinum strip. The heating effect of the impinging rays is measured by determining the change in resistance of the strip by means of a wheatstone bridge arrangement. The various portions of the spectrum are passed over the platinum strip by turning the prism. At the same time, the galvanometer deflection (proportional to the resistance change) is recorded on a moving photographic plate. Thus curves of spectral energy distribution are obtained, the abscissae giving the wave length of the radiation and the ordinates the intensity.

The latest discussion of errors of spectrobolometry made by Abbot is in volume 4, page 162. Some of the errors he mentioned are:

1. *Change of sensitiveness of apparatus during the observation.*—This error is now very small.

2. *Change of reflection of mirror with angle and with dust.*—The mirror is dusted before every run and its change of reflection with angle has not been detected with certainty. There is a possible, but probably small, error here.

3. *Imperfect following of the sun.*—Abbot says, " * * With our ordinary care in following, changes as great as 1 percent in bolometric response were likely to occur several times in a single bolograph. Greater care is now being practiced."

4. *Changes of sensitiveness with temperature or with changes of the earth's magnetic field are said to be probable.*

5. *Inequalities of the galvanometer scale.*—Successive bolographs are taken with a shift of one to two centimeters in zero point of the galvanometer scale, so that unevennesses will introduce errors. These inequalities "seldom exceed 2 percent on the length of the scale," and would not introduce more than, say, ½ percent error in any bolograph.

6. *Measuring ordinates.*—The average ordinate is less than 7 centimeters. As Abbot says: "One cannot reasonably hope the probable error of a single ordinate measurement is less than 1 percent." In determining a bolographic area, if all measurements were independent, one would get a probable error of $\frac{1}{\sqrt{38}}$ percent, if 38 places were measured. Abbot guesses the probable error in an area determination due to ordinate measurement to be 0.2 percent.

7. *Determination of band areas.*—Abbot estimates that the band areas are measured with a probable error of 1.5 percent. The band areas range from 8 to 25 percent of the areas remaining after their removal from the smooth curve areas. Therefore these errors will cause from 0.12 to 0.38 percent probable error in the total area.

8. *Correction factors.*—Two pyrheliometric observations are taken simultaneously with each bolograph. A correction factor is determined for each bolograph to make its area proportional to the observed pyrheliometric observation. Many of the above errors will tend to be eliminated or reduced by these correction factors. However, the correction factors have in them the pyrheliometric error of two observations, which may be taken as $\frac{0.25}{\sqrt{2}}=0.18$ percent.

9. *Area of energy curve outside atmosphere.*—The energy curve outside the atmosphere is gotten by a process of extrapolation. Each of the 6 bolographs made during a morning is divided into 38 vertical sections. The ordinate at the center of each section is taken as proportional to the energy in that section. The sum of the ordinates, weighted according to the width of the section, then, represents the total area of the curve, or the total energy. The present practice is to measure the ordinates directly from the bolographic curve, except in the infrared, where they are measured to a smooth line passing through the peaks of the curve and extending over the absorption bands. Formerly (vol. 4) they were measured to a smooth curve throughout the spectrum. Each measured ordinate is multiplied by a predetermined factor to correct it for the selective transmission of the optical system of the bolometer. All the ordinates of any one bolograph are then multiplied by a correction factor as described under No. 8 above. The final corrected ordinates are thus obtained. We shall call y_λ the corrected ordinate corresponding to the wave length λ. The logarithms of these ordinates are plotted against the air masses at which each bolograph was taken. For any one wave length these logarithmic plots should determine a straight line. The best straight line is passed through the points and the value of $\log y_\lambda$ at $m=0$ is taken as the logarithm of the ordinate of that wave length for the energy curve outside the atmosphere. We will call this extrapolated ordinate h_λ.

The equation of the best straight line is: $\log y_\lambda = \alpha + b\,(m-\overline{m})$, where $\overline{m}$ is the mean air mass used. α and b are determined by—

$$\alpha = \overline{\log y_\lambda} = \text{mean value of } \log y_\lambda$$

$$b = \frac{\Sigma\{(\log y_\lambda)(m-\overline{m})\}}{\Sigma m^2 - \frac{1}{n}(\Sigma m^2)}$$

By comparison with the theoretical equation, $\log y_\lambda = \log h_\lambda + m \log a_\lambda$, where a_λ is the atmospheric transmission coefficient for the wave length λ, we see that $\log h_\lambda = \alpha - b\,\overline{m}$ and that $b = \log a_\lambda$.

We shall now see how random errors in the corrected ordinates y_λ affect the atmospheric transmission coefficient a_λ, the individual extrapolated ordinates h_λ, and the total computed area outside the atmosphere, Σh_λ.

Let σ_λ = the standard deviation of the distribution of errors of $\log_{10} y_\lambda$. This will be $0.0043 \times$ the standard deviation, σ_y, of the percentage errors in y_λ. Or conversely, $\sigma_y = 230.3\,\sigma_\lambda$.

The standard error in α is $\frac{\sigma_\lambda}{\sqrt{n}}$, where n is the number of bolographs taken.

The standard error of b is $\frac{\sigma_\lambda}{\sqrt{\Sigma(m-\overline{m})^2}}$.

Atmospheric transmission coefficients

Since $b = \log a_\lambda$, this last expression is also the standard error of $\log a_\lambda$.

If $\sigma_{a\lambda}$ = standard percent error of a_λ—

$$\sigma_{a\lambda} = 230 \text{ (std. error of } \log a_\lambda) = 230\,\frac{\sigma_\lambda}{\sqrt{\Sigma(m-\overline{m})^2}}$$

$$= \frac{230}{\sqrt{\Sigma(m-\overline{m})^2}}\,(0.0043 \times \sigma_y)$$

or

$$\sigma_{a\lambda} = \frac{\sigma_y}{\sqrt{\Sigma(m-\overline{m})^2}}$$

Extrapolated ordinates

Since $\log h_\lambda = \alpha - b\,\overline{m}$, the standard error of $\log h_\lambda$ is—

$$\sqrt{\sigma_\alpha^2 + \overline{m}^2\sigma_b^2} = \sqrt{\frac{\sigma_\lambda^2}{n} + \overline{m}^2\frac{\sigma_\lambda^2}{\Sigma(m-\overline{m})^2}} = \sigma_\lambda\sqrt{\frac{\Sigma m^2}{n\Sigma m^2 - (\Sigma m)^2}}$$

The percent standard error of h_λ is then—

$$\sigma_{h\lambda} = 230\sigma_\lambda\sqrt{\frac{\Sigma m^2}{n\Sigma m^2 - (\Sigma m)^2}}$$

TOTAL OUTSIDE AREA

As the total outside area is Σh_λ, the percent error of A is—

$$\sigma_A = \frac{1}{\Sigma h_\lambda}\sqrt{\Sigma h_\lambda^2 \sigma_{h\lambda}^2}$$

$$= 230\sqrt{\Sigma_\lambda\left(\frac{h_\lambda}{\Sigma h_\lambda}\right)^2 \sigma_\lambda^2\left(\frac{\Sigma m^2}{n\Sigma m^2 - (\Sigma m)^2}\right)}.$$

There are two methods available for estimating σ_λ. One is to measure directly the deviations of the observed values of $\log y_\lambda$ from the best straight line passed through them. If there are n points determining the line, then the best estimate of σ_λ is from $\sigma_\lambda^2 = \frac{\Sigma(\delta \log y_\lambda)}{n-2}$, where the $\delta \log y_\lambda$'s are the measured deviations. On page 166, Volume 4, are given the percentage departures of the

y's from their straight line plots of the observations at Calama, Chile, for August 1, 1919. I have converted these back to deviations in $\log_{10} y_\lambda$, as that is how they were measured, and calculated σ_λ from them. All ordinates will not have the same percent standard deviation, as they are not of the same length. Since σ_λ is proportional to the percent error, it also will not be the same for all ordinates. For the ordinates from places

−5 to −14, inclusive, I find $\sigma_\lambda = \sqrt{\dfrac{\Sigma(\delta \log y_\lambda)^2}{15(n-2)}} = 0.00316$,

corresponding to a percent standard deviation of 0.73 percent in the ordinates. These are the largest measured ordinates and should give the minimum standard deviation. For places −16 to −23, inclusive, $\sigma_\lambda = 0.00619$, or $\sigma_\lambda = 1.43$ percent. For places +3 to −4½, inclusive, $\sigma_\lambda = 0.0162$, or $\sigma_\lambda = 3.72$ percent. Taking all ordinates lumped together, $\sigma_\lambda = 0.00964$, or $\sigma_\lambda = 2.22$ percent. As these errors enter into the total area weighted by the length of the ordinate, the smallest of these standard deviations will be weighted the most.

We can get another estimate of σ_λ if we can get an estimate of the percent error of a_λ, the atmospheric transmission coefficient, for the two were shown to be related by—

$$\sigma_{a\lambda} = 230 \frac{\sigma_\lambda}{\sqrt{\Sigma(m-\overline{m})^2}}$$

The values of a_λ are given for various wave lengths for each day that the long method of computation was used. It will be safe to assume, I think, that except for accidental errors of the kind we wish to evaluate, the ratio of atmospheric transmission coefficients for two neighboring ordinates will bear a constant ratio to each other, except for a slight trend of their ratio depending on their magnitude. The deviations from a regression line of ratio on magnitude will give a measure of the accidental errors of measurement of the transmission coefficients.

Calling the sum of $a_{\lambda 1} + a_{\lambda 2} = s$, and the ratio $\dfrac{a_{\lambda 1}}{a_{\lambda 2}} = r$, we can calculate the standard deviation σ_r of the ratios from a straight line relation with s as follows:

$$\sigma_r^2 = \frac{\Sigma r^2 - \dfrac{1}{n}(\Sigma r)^2 - \dfrac{\left(\Sigma sr - \dfrac{1}{n}\Sigma s \Sigma r\right)^2}{\Sigma s^2 - \dfrac{1}{n}(\Sigma s)^2}}{n-2}$$

For the 59 long method values for June-August, 1919, the value of σ_r obtained is $\sigma_r = 0.491$ percent.

Now this value is a combination of the independent errors in two neighboring atmospheric transmission coefficients. Denoting the standard deviation of these independent errors by σ_t, we have $\sigma_r = \sqrt{2}\sigma_t$, or $\sigma_t = 0.347$ percent. There are two reasons for believing that the total error of two neighboring atmospheric transmission coefficients will not be independent. In the first place, such errors as nonuniformity of the galvanometer scale and imperfect following of the sun are likely to be carried through a fairly large region of the spectrum, so that the errors of nearby ordinates are not independent for this reason. Secondly, the method of measurement at this time (1919) was to pass a smooth curve through the bolograph trace and to measure to this smoothed

curve. The main error of measurement then was the placing of the line, measurements to it being fairly accurate. If a line is in error at one point, it is probable that it will be in error in the same sense at a neighboring point.

Of late the measurements of ordinates throughout a large part of the spectrum have been made directly to the original bolographic trace, eliminating the last of the two reasons for dependence of neighboring ordinates given above. I have calculated the value of σ_r for the 47 atmospheric transmission coefficients of wave length 0.621 μ and 0.499 μ given for the year 1930 at Montezuma. These wave lengths are in the part of the spectrum in which the measurements are made directly to the bolographic curve. I get $\sigma_r = 0.643$ percent and a consequent σ_t of 0.455 percent.

To get the standard error of individual bolographic ordinates from this we use—

$$\sigma_y = \sqrt{\Sigma(m - \overline{m})^2}\,\sigma_t$$

For an average day at Calama for June-August, 1919, the factor $\sqrt{\Sigma(m-\overline{m})^2}$ was 2.42. For an average day of 1930 it was 2.85. Use of these factors gives the following values of σ_r, σ_t, and σ_y.

	Percent	Percent	Percent
Calama, July–August, 1919	$\sigma_r = 0.491$	$\sigma_t = 0.347$	(A) $\sigma_y = 0.841$
Montezuma, 1930	$\sigma_r = 0.643$	$\sigma_t = 0.455$	(B) $\sigma_y = 1.30$

To summarize, the two figures in the last column are the standard deviations of the errors in the bolographic ordinates that are not carried over from one ordinate to the neighboring one. The value (A) was obtained for a time when measurements were made to a smoothed curve. The only carry over of errors in (B) is the instrumental one. These are to be compared with the value, due to all accidental errors, obtained above from the deviations log y for the longer ordinates from the best straight-line plots against m for August 1, 1919. The value obtained was $\sigma_y = 0.73$ percent. This value is not significantly different from (A) but is less than (B). The reason for this is perhaps that August 1, 1919, was an "excellent" day, as explained below.

Total area of bolographic curve outside atmosphere.—From the table of departures of log y from straight-line plots given for August 1, 1919, at Calama, we can calculate the standard error of the total area of the curve outside the atmosphere for that day. The bolometry of that day was graded "excellent." There were only five days that were marked "excellent" in July and August, 1919, out of 16 days of long-method observation. As the grade is determined "mainly from the approximation of the logarithmic plots to straight lines" (vol. 4, p. 130), it is apparent that August 1 is one of the best 30 percent of days as far as lack of errors go. To get the total error we use:

$$\sigma_A = 230 \sqrt{\Sigma_\lambda \left(\frac{h_\lambda}{\Sigma h_\lambda}\right)^2 \sigma_\lambda^2 \left(\frac{\Sigma m^2}{n\Sigma m^2 - (\Sigma m)^2}\right)}.$$

The values of m used are taken from table 28, page 123, volume 4. The weights $\dfrac{h_\lambda}{\Sigma h_\lambda}$ were calculated from column 11, and columns 2 and 3, table 58, page 203, volume 4. Using these values I get the standard error of the extrapolated area to be $\sigma_A = 0.311$ percent. This gives a probable accidental error of P. E. = 0.21 percent for the area of the solar-energy curve outside the atmosphere for August 1, 1919, one of the better days there.

Supposedly by a similar method Abbot comes to the conclusion (vol. 4, p. 167), "For long-method values at Calama we find a probable error of 0.12 percent for the solar constant caused by errors of the bolographic ordinates." This seems unreasonable in view of the 0.21 percent calculated above, applying to one of the best days.

For a day (September 20, 1914) at Mount Wilson Abbot gives, in volume 4, page 340, a series of actual bolometer readings, together with the transmission factors and the pyrheliometry correcting factors, so that nearly complete calculations can be carried out for this day. For the last 6 bolographs, corresponding to a usual day's run, I have calculated by least squares the sum of the deviations of the ordinates from the best straight-line logarithmic plots for each of the 38 places measured on the bolographs. The formula used was:

$$\Sigma(\delta \log y)^2 = \Sigma(\log y)^2 - \frac{1}{n}(\Sigma \log y)^2$$
$$-\frac{\left\{\Sigma m \log y - \frac{1}{n}(\Sigma m)(\Sigma \log y)\right\}^2}{\Sigma m^2 - \frac{1}{n}(\Sigma m)^2}$$

Making the same calculation as above for total error we get $\sigma_A = 0.2443$ percent and P. E.=0.165 percent, for the standard error and probable error of the area of the bolometric curve outside the atmosphere, if it were calculated by least squares. This does not include the errors of constructing the graphs and reading off the ordinates. Except for the last fact, this is in good agreement with the value used by Abbot for Mount Wilson. He uses a probable error of 0.17 percent. This day, September 20, 1914, was considered "excellent" as far as bolometric errors go. It is reasonable to assume that for an average day a higher error is to be expected. Also it is certain that the graphical method will yield greater errors than the least square method. A comparison of the P. E. found for this day, 0.165 percent, and of the P. E. of 0.21 percent found for an excellent day at Calama by the graphical method shows a difference that might reasonably be due to the graphical errors. It will be simple and reasonable to take for the probable error of bolographic area outside the atmosphere a value of 0.20 percent for excellent days and of 0.25 percent for an average day for either station. The results on the atmospheric transmission coefficients for 1930 seem to indicate that the errors at least are no less now, and may be more.

In addition to this error, the long method includes the error of the average of two pyrheliometer readings and the errors due to changing atmospheric transparency, to be treated later.

III. SOURCES OF ERROR IN THEORY AND METHOD

A. INFRARED CORRECTION

The spectrobolometer indicates the spectral energy curve only between the wave lengths 2.5 microns and 0.346 microns. Outside of these limits the sun's energy must be estimated and added to the area under the bolographic curve.

For the infrared, the estimate is made from measurements made at Mount Wilson in 1922 with a rock salt prism. The infrared correction is given as a percent of the bolographic area from 2.5 to 0.704 microns. The percent taken varies with the amount of "precipitable water" and amounts to about 2 percent of the total

bolographic area outside the atmosphere. The correction is probably not very accurate, but as it is a percentage correction inaccuracies in it will not affect variations of the solar constant.

B. ULTRAVIOLET CORRECTION

The ultraviolet correction is based on the average of two solar energy spectra, one assumed from a 6,000° black body radiation and the other one as determined experimentally. The ultraviolet corrections determined from these differ by a factor of nearly two, so that their average cannot be regarded as accurate. The correction is taken as a certain percent of the area of the spectrobolograph included between the wave lengths of 0.346 microns and 0.704 microns, the percent depending upon the air mass and the atmospheric transmission coefficient (table at top of p. 109, vol. 5). The correction taken amounts to 3.44 percent of the solar energy at zero air mass, i. e., outside the atmosphere. The probable error of this, in the sense of being the error that is as likely to be exceeded as not, is about 1 percent (of the total solar energy). Provided that the method of applying the correction is correct, the error will not introduce errors in the variability of the sun, as it is a percentage correction.

However, there is evidence that if the sun is not constant, most of the variation occurs in the ultraviolet. Abbot has plotted the ratio of each bolographic ordinate on days of high calculated solar constant to the corresponding ordinate on days of low solar constant value. The resulting ratios are plotted as a function of the wave length of the ordinate, and a plot is shown on page 29 of volume 5. (The same graph is shown in vol. 80, No. 2, and vol. 77, No. 5, of the Smithsonian Miscellaneous Collection. The latter reference gives the best explanation of the curves.) It is to be seen that days of high values of the solar constant have more radiation in the ultraviolet than days of low solar constant. Corresponding to a 2.3 percent increase in observed solar constant, in the mean there is a 30 percent increase of radiation of wave length 0.35μ, a 20 percent increase of radiation of wave length 0.385μ, a 10 percent increase of radiation of wave length 0.44μ, a 5 percent increase of radiation of wave length 0.5μ, and no increase of wave length longer than 0.7μ.

The curve C, for Montezuma short interval fluctuations, does not show such a large increase in the ultraviolet, and shows more in the shorter visible wave lengths, though the fluctuations are such as to make the course of the curve indeterminate.

Abbot has used this spectral variation of solar intensity as evidence for the reality of the solar variations. However, I do not find this evidence convincing. In the first place, if the reported variations of solar constant were actually due to changes in atmospheric conditions affecting the measurements, most of the variation would be in the ultraviolet, just as is found experimentally. The reason for this is that the effect of the atmosphere in depleting the solar radiation is much more severe in the violet and ultraviolet. If there is any correlation between atmospheric transparency and solar intensity, the variation of solar intensity produced by a change of atmospheric transparency must be most pronounced in the violet and ultraviolet, because the atmospheric transparency goes through the widest variations in this region. In the following table, in the column marked A are given the percent variations found at the various wave lengths corresponding to a drop of 2.3 percent in S. C. observed at Harqua Hala in 1922. B gives very roughly the fraction

of the total solar energy of a given wave length absorbed in the atmosphere, at air mass 1.5. It is seen that the larger the effect of the atmosphere the greater is the change of measured solar intensity.

λ	A	B
	Percent	
0.35μ	30	0.47
0.385μ	20	.38
0.44μ	10	.26
0.5μ	5	.19
0.7μ	0	.07

Instead of considering the large variation in the ultraviolet as corroborative evidence of real solar variability, one could take it as evidence that the drop of 1922 was largely due to atmospheric changes.

In support of the reality of the large variations in the ultraviolet, Abbot cites the work of Pettit.[2] (Astrophysical Journal *75*, 185 (1932).) Pettit measures, by means of a thermocouple, the ratio of the solar energy transmitted by a silver film to that transmitted by a gold film. The gold film transmits a band in the green, while the silver transmits a narrow band from 0.31μ to 0.33μ in the ultraviolet. Large variations in the ratio are found, the extreme values in the 7-year period 1924–31 being 0.95 and 1.57 on an arbitrary scale. Since the green light is shown to be nearly constant by Abbot's work, variations in the ratio are to be attributed to variations in the ultraviolet. The extreme range is then 50 percent of the mean value. A daily variability of 5 percent was reported.

The monthly curve of Pettit's ratios seems to show some relation to a curve of sunspot numbers. This would again suggest a relation to ozone, as the amount of ozone is shown to be correlated somewhat with sunspot numbers. In any case, Pettit's curve shows little correlation to the solar constant curve of Abbot, and therefore cannot be taken as evidence of the reality of the reported solar variations.

Bernheimer (Monthly Weather Review *57*, 412, 1929) analysed the first 2 years of Pettit's work and casts some doubt upon its validity. He points out the decided seasonal march of measured ultraviolet radiation and compares it to a curve of atmospheric turbidity factors. He says: "There is temptation to assume that the observed changes in the ultraviolet do not take place in the sun, but reflect processes in the earth's atmosphere."

Dobson (Proc. Roy. Soc. *A 104*, 252, 1923) made some early studies of the variation of the sun's ultraviolet. His measurements were made photographically, the pictures being taken through a silver screen. He got a standard deviation of daily values of 30 percent, with errors estimated at less than 20 percent. This would leave a daily variability of solar ultraviolet of more than 20 percent, as compared to Pettit's daily variability of 5 percent. The discrepancy indicates terrestrial influences on one or both measurements.

These measurements on the ultraviolet indicate a possibility (though not a certainty) that there are relatively wide fluctuations in the sun's intensity in the ultraviolet region of wave lengths 0.31μ to 0.33μ. Abbot's measurements, which extend down to 0.346μ, indicate (p. 29, vol. 5) that if the measured solar variability exists, most of it takes place in the violet end of the spectrum.

The two results are compatible, though they cannot be explained by a temperature change of the sun. And it must be borne in mind that if the solar variability measured by Abbot is caused by variations of atmospheric quality, most of the observed variability would still be in the violet.

However, if the solar variability is real and most of it takes place in the violet and ultraviolet, then the method of applying the ultraviolet correction will give too low a range to the recorded solar variability. A one percent change in solar energy of wave length longer than 0.346μ would give no change in the part of the spectrum comprising the longer wavelengths, and a 2 percent change in the short wave length part down to 0.346μ. The ultraviolet correction would be changed by 2 percent then, as it is taken as 6.88 times the energy in the lower half of the spectrum. The change in the ultraviolet correction applied would then be only 0.02×6.88=0.1376 percent, and the total recorded variability would be 1.14 percent of the solar constant. Curve B of figure 1, page 29, volume 5, indicates, however, that the ultraviolet beyond 0.346μ will change by 20 percent (at Harqua Hala) for a one percent recorded change in solar constant. Therefore the real change in the ultraviolet beyond 0.346μ would amount to 3.44 times 1.14 times 0.20=0.78 percent of the solar constant, and the real change of solar constant would be 1.78 percent instead of the 1.14 percent recorded. It is seen then that if the solar variations are real and confined to the violet end of the spectrum, the recorded range of solar variation is only about 65 percent of the true range of variation.[3]

C. OZONE CORRECTION

The ozone of the atmosphere absorbs sunlight in three spectral regions. In the extreme ultraviolet the absorption is strong, but since sunlight is weak there, the effects of varying amounts of ozone may not be important. In the spectral region λ=0.48μ to λ=0.63μ there is a weak absorption band, but it is in the most intense region of the spectrum. Variations of ozone here will affect the total solar energy received at the surface of the earth quite appreciably, by amounts up to more than 1 percent. In the extreme infrared there is a third band. Varying amounts of ozone will not affect the solar constant as obtained by the long or bolometric method, but will affect the short method directly. For this reason, corrections are determined for the amount of ozone and are applied to the short method values at Table Mountain. According to Abbot (vol. 5, p. 125): "Fortunately the variation of atmospheric ozone at the receiving station on Mount Montezuma, Chile, is so small that the error is negligible." No correction is applied to Montezuma values for this reason.

The determination of the amount of ozone present depends upon the relative absorption of the atmosphere for two nearby wavelengths. Dobson, P. R. S., *A, 129*, 467 (1929), measures photographically the relative intensity of wave lengths in the ultraviolet, about λ=0.303μ and λ=0.325μ. The former of these is in the strong ozone absorption band, while the latter is nearly out of it.

The ozone is measured by Dobson in units of the equivalent thickness that the ozone would have if it were all concentrated in one pure layer at normal pressure and temperature. The values found range around 0.3 centimeter. The probable error of one measurement in

[2] Since this paper was written, the following notice appeared in the Quarterly Bulletin on solar activity of the International Astronomical Union, No. 45, Jan.–Mar., 1939: "The intensity numbers for ultraviolet radiation will no longer be published in the Bulletin either. Mr. Edison Pettit, who made these measurements, has written us on the subject as follows: 'As the work has now covered a period of 15 years and seems to show only a seasonal term due to atmospheric ozone we are discontinuing the observations.'"

[3] Since this paper was written, solar constant values are being revised on the basis of similar considerations arrived at independently by Abbot, assuming that the variations in the ultraviolet represent solar changes.

Europe by Dobson is 0.005 centimeter. At Arosa (Swiss Alps) the change throughout the day is often less than 0.003 centimeter.

Dobson says:

The (daily) values for Montezuma are not given as the changes from day to day are so small that it is not possible to be sure of their reality. * * * The long double journey through the tropics is definitely harmful to the photographic plates, and the excessive dryness and warmth caused appreciable distortion of the wooden spectrograph. For these reasons one would not place such reliance on the accuracy of the results obtained at Montezuma as on those obtained in Europe. This, however, cannot invalidate the result that the ozone is extremely constant there. There are also signs of a curious small diurnal variation not found at other stations which makes it difficult to assign accurate mean values to each day.

The monthly values he gives for Montezuma, from November 1926 to October 1927, are 0.223, 0.218, 0.212, 0.208, 0.215, 0.210, 0.209, 0.213, 0.216, 0.224, 0.222, 0.225. This shows a range of from 0.208 to 0.225 centimeter of ozone, as measured by Dobson.

Cabannes and Du Fay (J. d Phys. et le Rad. *8*, 353, 1927) calculate the amount of ozone by plotting the logarithms of the atmospheric transmission coefficients as determined by the Smithsonian Institution against the inverse fourth power of the wave length. Departures from a linear relation in the region 0.48μ to 0.65μ were taken as due to ozone absorption, and the amount of departure was converted into equivalent centimeters of ozone. For Montezuma, from July 1918 to July 1920 they found the amount of ozone nearly constant at about 0.3 centimeter of ozone. There was no annual period observed, but the amount varies between extremes of 0.33 centimeter in July 1918 to 0.26 centimeter in May 1919. The corresponding variation at Mount Wilson was found to be from about 0.24 to 0.36 centimeter of ozone.

Fowle, of the Smithsonian Institution (Sm. M. C., vol. 81, No. 11, 1929), uses the bolometer results to calculate the monthly variation of ozone. He says that Cabannes and Du Fay's work is inaccurate, due to the use of values not corrected for water vapor and because changing atmospheric transmission affects their results. He criticises Dobson's work, implying that a variation with water vapor or atmospheric pressure is introduced in the method of calculation, and disagrees with the use of Bouger's formula of extrapolation for the wave lengths Dobson uses.

Fowle plots *a*, the atmospheric transmission coefficient, against the prismatic deviation. He gets a deviation from a smooth curve in the region of ozone absorption. If the smooth curve value is a_a and the observed value is *a*, and if a_o=atmospheric transmission coefficient of ozone, then $a_o=\dfrac{a}{a_a}$. If *e* is the solar energy for the wave length considered, then $\Sigma\dfrac{a}{a_a}e$=energy absorbed by ozone. This is summed over places 22, 24, and 26. "The accuracy cannot exceed 1 part in 30" for a single day's determination, assuming no accidental error or changing ozone.

Fowle finds a wider variation in ozone content than does Dobson. For 22 daily results at Table Mountain given graphically there is a standard deviation of 0.062 centimeter from a mean of 0.264 centimeter, by Fowle, while for a corresponding period Dobson's values have a standard deviation of 0.0053 centimeter from a mean of 0.221 centimeter. In other words, Fowle's values have about 12 times the daily variability that Dobson's have. Fowle gives monthly values of ozone in terms of the energy that is absorbed from the sunlight at air mass 1. The variability at Montezuma is less than that at Table Mountain. The mean annual difference from the maxi-

mum month to the minimum month is 0.0021 calories at air mass 1, or about 0.1 percent of the solar constant. At air mass 2 it would be about 0.2 percent. In an earlier paper by Fowle (Terr. Mag. and Atm. Elect. *33*, 151 (1928)) a few individual values are plotted, indicating that changes of this magnitude can occur in the course of a month or so.

The method used to correct short-method values at Table Mountain is due to Abbot (vol. 5, p. 126). The ratios of the bolographic ordinates of a single day to those of a standard day are plotted against the prismatic deviation of the ordinate. These will be roughly on a straight line except for the ozone region. Here the ratios will be above or below the straight line according to whether there is more or less ozone than on the standard day. The deviations are measured for two ordinates and two air masses, when possible. Assuming a standard error of 0.75 percent in determining a single bolographic ordinate, the standard error of determination of the ozone correction will be 0.07 percent of the solar constant. This will appear as a direct error in the solar constant, assuming the correction factors to be right.

By a comparison with Dobson's ozone values at Table Mountain it can be shown that the annual variation of 0.017 centimeter ozone measured by Dobson at Montezuma corresponds to a fluctuation in solar constant values by the short method due to ozone of about 0.09 percent. As measured by Fowle it is about 0.2 percent, or possibly more. The daily fluctuation of ozone is not given by Dobson. The diurnal variation which he mentions might possibly be due to a pressure effect.

Summary.—The uncertainty of determination of the ozone correction at Table Mountain and at Harqua Hala introduces a standard error, σ, of at least 0.07 percent in the daily short method values. An annual variation of 0.1 to 0.2 percent or more in monthly values of the solar constant determined by the short method is to be expected at Montezuma due to lack of application of a correction for ozone. Changes of this magnitude can occur from month to month.
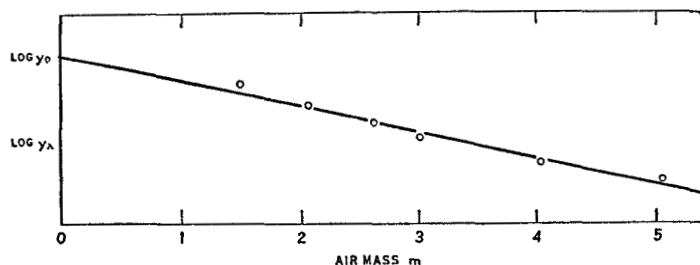

FIGURE 4.—Method of extrapolation of intensities to zero air mass.

D. CHANGES IN ATMOSPHERIC TRANSPARENCY

The long or bolometric method of determining solar constants depends upon observations extending over a period of about 2½ hours. Changes in atmospheric transparency during this time will introduce error in the solar constant. This effect has long been recognized. H. Knox-Shaw considered its effect in some detail in 1915. (Bulletin No. 17 of the Helwan Observatory, Cairo, Egypt.) He concludes that "even if R (solar constant) is really entirely constant, a progressive change in the atmospheric transmission during the period, which is such as not to destroy the linearity of the relationship between log R_m and *m*, if present on a sufficient number of days, will cause a large negative correlation between R

and a, and R will apparently vary owing to variations in a." How this comes about is as follows:

The intensity of sunlight of a given wave length at the outside of the atmosphere is determined by plotting the logarithm of the intensity of that wave length at several air masses against the air mass and extrapolating by means of a straight line to zero air mass. Figure 4 illustrates the procedure. y_o is the intensity of the particular wave length outside the atomsphere and the slope of the line is $\log a_\lambda$, where a_λ is the atmospheric transmission coefficient for wave length λ. The slope of the line is always negative, as lower intensities are measured at higher air masses. This gives a value of a_λ which is less than one, unity corresponding to perfect transparency.

Now if we consider a single wave length and assume that the solar energy does not change during the observation, the position of $\log y_o$ will be fixed. If a value y_5 is
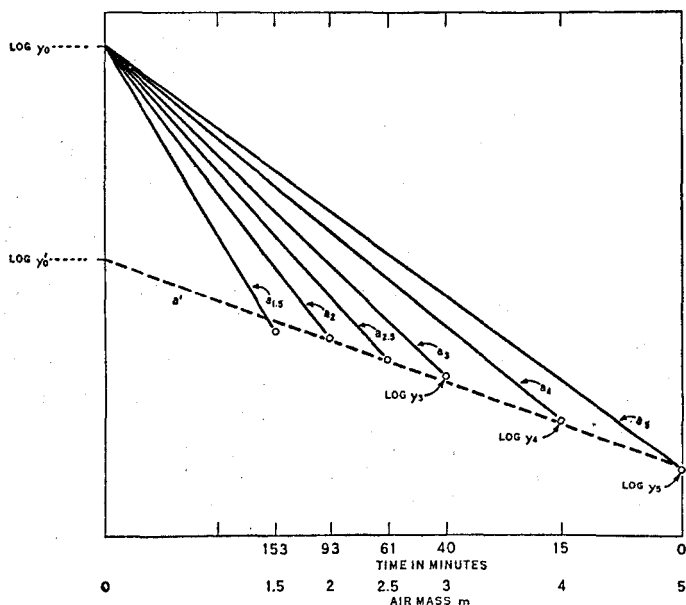
FIGURE 5.—Error in extrapolation due to changing atmospheric transmission.

observed for the intensity at air mass 5 early in the morning, the line through $\log y_o$ and $\log y_5$ determines the atmospheric transmission coefficient a_5 at that time. If the atmospheric conditions do not change, all succeeding logarithms of the observed intensities will lie on the line of slope $\log a_5$. If, however, the atmosphere becomes less transparent before the next observation is taken, the logarithm of the observed intensity will lie below the line of slope $\log a_5$, and with $\log y_o$ will form a new line of slope $\log a_4$, where a_4 is the atmospheric transmission coefficient at the time of the observation at air mass 4. If the transparency continues to decrease throughout the morning, each value of $\log y$ will fall below its expected position, and a series of observations such as those given in the circles in figure 5 are obtained. When a straight line is passed through them, and extrapolated to zero air mass, it will give a value of $\log y_o'$ below the true one, and a low value of the solar constant will be reported for that day.

If we assume a constant rate of change of the atmospheric transmission coefficient during the morning, for small changes this will be a constant percentage change in a and consequently a constant rate of change of $\log a$, the slope of the line. If the time is measured from the taking of the first bolograph at $m=5$, and if k is the decrease in $\log a$ per unit time, then the slope at any time=

$\log a_5 - kt$, and any ordinate is given by $\log y = \log y_o + (\log a_5 - kt)m$. The deviation from the line through $\log y_o$ and $\log y_5$ is $-ktm$. The approximate times from air mass 5 to each air mass for September 20, 1914, at Mount Wilson are given at the bottom of the graph in Figure 5, and the values of $\log y$ to be expected at the various air masses for an arbitrary value of k are shown. It is to be seen that the values of $\log y$ obtained for a constantly changing air transparency lie near to a straight line, and that the value of $\log y_o'$ obtained from this line is in bad error. Also the value of the atmospheric transmission coefficient recorded for that day will be in error, being higher than the transparency was at any time during the morning.

A brief investigation shows that it is in general true that a uniformly changing atmospheric transmission coefficient will give nearly a straight line plot of values of $\log y$. For a spot under the solar path, the time will be proportional to the zenith angle Θ. If $\Theta_5=$the zenith angle for air mass 5, then the time measured from air mass 5 will be—

$$t=c(\Theta_5-\Theta), \text{ where } c \text{ is a constant}$$

but— $\sec\Theta=m$, or $\Theta=\sec^{-1}m$

therefore $t=c(\sec^{-1}5-\sec^{-1}m)$

If we use a series approximation for $\sec^{-1}m$, we get—

$$t=c\left(\sec^{-1}5-\frac{\pi}{2}+\frac{1}{m}+\frac{1}{6m^3}+\frac{1.3}{2.4.5m^5}+\ldots\right).$$

We now put this value of t in the above equation for $\log y$ with constantly changing a, namely:

$$\log y=\log y_o+(\log a_5-kt)m$$

and get—

$$\log y=\log y_o+\left(\log a_5-kc\left\{\sec^{-1}5-\frac{\pi}{2}+\frac{1}{m}+\frac{1}{6m^3}+\ldots\right\}\right)m$$

or

$$\log y=\log y_o+\left\{\log a_5-kc\left(\sec^{-1}5-\frac{\pi}{2}\right)\right\}m-kc$$
$$-kc\left\{\frac{1}{6m^2}+\frac{3}{40m^4}+\ldots\right\}.$$

If we put—

$$\log a_5+kc\left(\frac{\pi}{2}-\sec^{-1}5\right)=\log a'$$

and—

$$\log y_o-kc=\log y_o'$$

Then

$$\log y=\log y_o'+m\log a'-kc\left\{\frac{1}{6m^2}+\frac{3}{40m^4}+\ldots\right\}$$

Except for the term $-kc\left\{\frac{1}{6m^2}+\frac{3}{40m^4}+\ldots\right\}$ the expression is linear in m and $\log y$. This term is small and rapidly approaches zero for large m. Thus for air mass 2 the first term of the correction becomes $-kc/24$. k is the decrease in $\log a$ per unit time, or 0.0043 times the percent decrease in a per unit time. c is the time for a unit zenith angle. If the time is expressed in hours, $c=\frac{24}{2\pi}=3.82$ hours/radian. Then $kc=0.0165$ times the percent decrease in a per hour. Therefore for $m=2$ the first term of the correction to a straight line is 0.00069 in $\log y$, or -0.16 percent in y, this is for a 1 percent decrease in a per hour. For $m=4$ this

reduced to —0.04 percent in y. Thus we see that if the percent change in atmospheric transmission coefficient is uniform, the values of log y will be on a straight line, except for small deviations at low air mass. If the atmospheric transmission is decreasing, the values of log y will lie slightly below the line at low air mass, while if it is increasing, they will be slightly above it.

We see from the calculations above that the error introduced in log y_o is —kc, or the percent error in y_o is $230 \times 0.0165 = 3.82$ times the percent change of a per hour.

The average value of log a for the day is—

$$\frac{\log a_5 + \log a_{1.5}}{2} = \frac{\log a_5 + \log a_5 - kt}{2}$$

$$= \log a_5 - \frac{kt}{2} = \log a_5 - \frac{1}{2}kc\{\sec^{-1}5 - \sec^{-1}1.5\}$$

The observed value, log a', is $\log a + kc\left\{\frac{\pi}{2} - \sec^{-1}5\right\}$

The error in log a is the difference log a' minus the average log a for the day. This difference is—

$$\frac{1}{2}kc\{\pi - \sec^{-1}5 - \sec^{-1}1.5\} = 0.4657\ kc$$

As kc is 0.0165 times the percent decrease in a per hour, the error in log a is 0.4657 times 0.0165 or 0.00768 times the percent change in a per hour. The percent error of a' is 230 times the error in log a, or 1.74 times the percent decrease in a per hour.

These figures are based on a tropical sun, where the zenith angle is equal to the hour angle. For higher latitudes, the error in the same air mass range is greater, because a longer time is involved. For example, at Mount Wilson on September 20, 1914, the time from air mass 5 to air mass 1.5 was about 2½ hours, while at the Equator it is about 2 hours only. As average figures we can take—

$$\frac{\delta y_o}{y_o} = 4.75\frac{\delta a}{a}$$

per hour for the fractional error introduced in y_o by a fractional change in a per hour.

Similarly, the average error in the measured a' will be—

$$\frac{\delta a'}{a_{av}} = -2.18\frac{\delta a}{a} \text{ per hour.}$$

A glance at these two expressions will enable us to see at once that we must expect a negative correlation between the recorded values of changes of solar constant and of changes of atmospheric transmission coefficient, as an increasing transmission will cause a low value of the atmospheric transmission coefficient and a high value of the solar constant to be recorded.

The data for 1925 were used to get the effect of changing atmospheric transmission, as there are more long method determinations at Montezuma for this year than for any succeeding year. The apparent atmospheric transmission coefficients for this year are given in Monthly Weather Review, Supplement No. 27, 1926, but the figures given were not found satisfactory for use. They were determined from the logarithmic plots of the pyrheliometer readings, given in the same reference. A few trial plots indicated to me that the determinations of the values of a given there were based largely on the lower air masses. A comparison with a few values calculated by least squares indicates not only differences in magnitude but differences in daily changes, as the following series of 5 days shows:

| Tabulated a | 0.915 | 0.901 | 0.914 | 0.911 | 0.913 |
| Least squares a | .924 | .921 | .929 | .920 | .917 |

To get a measure of a that is closer to that actually used in the calculation of solar constant, the transmission cofficients for individual wave lengths were used. These are given for 10 wave lengths in volume 5, page 172. Each coefficient was weighted according to the intensity of the radiation in that position and the width of the spectral band that it represented. The weights arrived at are as follows:

| Place | 46 | 38 | 32 | 28 | 22 | 19 | 17 | 14 | 11 | 6 |
| Weight | 913 | 1,666 | 3,572 | 3,410 | 4,623 | 5,724 | 2,821 | 2,643 | 3,818 | 4,670 |

The sum of the weights is 33,830, and the general atmospheric transmission coefficient was taken as:

$$a = \frac{\Sigma a_\lambda w_\lambda}{\Sigma w_\lambda}.$$

The correlation between the atmospheric transmission coefficients and the values of solar constant given for the 100 days of 1925 is —0.02 ± 0.10 (= σ), certainly not significant. However, any real relation is masked by a yearly change of atmospheric transmission coefficient of large amplitude.

The correlation between the 55 daily changes of atmospheric transmission coefficient and the changes of solar constant was found to be —0.483 ± 0.104 (= standard error).

The root mean square daily difference of atmospheric transmission coefficient was 0.504 percent. It is doubtful if a fair estimate of the change in atmospheric transmission taking place during a 2½-hour morning observation can be obtained from this figure. For as H. Knox-Shaw says (Bulletin of the Helwan Observatory No. 23, (1921)), "* * * It would be surprising if the transmissive power of the atmosphere did not suffer diurnal variation as all other meteorological elements do."

From an abstract (Monthly Weather Review *57*, 412, 1929) of W. E. Bernheimer's article on "Radiation and Temperature of the Sun" in the Handbuck der Astrophysik, IV, 1929, we quote, "As cause (of the relation of atmospheric transmission coefficients to solar constant) there is pointed out the changing transparency of the air during the individual series of bolometric readings as is especially perceptible in the morning hours."

Any diurnal effect of changing atmospheric transmission such as this would not wholly be reflected in the day-to-day differences in recorded atmospheric transmission coefficient. However, the two extreme figures can be set for the effect of progressive changes from one day to the next on the solar constant. For a minimum value we can assume that the change is uniform and that the change for 1 hour is 1/24, or 0.0417 times the daily change, of 0.504 percent. Thus for a minimum root mean square hourly change in a we get 0.021 percent.

If we assume that the change from day to day is made up of many minute changes of positive and negative character, then the change for a given length of time is proportional to the square root of the time. Thus the average hourly rate for a 2½-hour observation would be

$$\frac{1}{2.5} \times \sqrt{\frac{2.5}{24}} \times \text{the daily change, or 0.0651 percent per hour.}$$

The error in solar constant due to each of these rates is gotten from an expression given above:

$$\frac{\delta y_o}{y_o}=4.75\frac{\delta a}{a}.$$

The standard error in the long method solar constant corresponding to the first estimate of changing a is 0.10 percent, and to the second is 0.31 percent. That is, if we neglect diurnal fluctuations of atmospheric transmission, we can say that the standard error in long method solar constant due to day-to-day changes of atmospheric transmission lies between 0.10 percent and 0.31 percent.

By knowing the theoretical relation between the increase in solar constant and the decrease in recorded atmospheric transmission coefficient caused by a progressive increase in transmission, we can get from the correlation coefficient an estimate of the fraction of the variance of solar constant values that is due to changing atmospheric conditions. For this purpose the values of the solar constant and atmospheric transparency have been grouped by months, and the departures from monthly means correlated. This gives a measure that is more independent of the wide yearly swings of atmospheric transmission and gives a fair approximation to the relation of daily changes.

The data were not grouped strictly by months, but nearly so. They were grouped so that consecutive daily determinations should all fall in one group. The extreme dates in each group are given below, together with the number of observations in each group.

Jan. 3–24	9
Feb. 17–Mar. 6	5
Mar. 11–18	7
Mar. 23–Apr. 8	17
Apr. 13–23	7
Apr. 30–June 3	20
June 16–July 3	14
July 13–Aug. 7	8
Sept. 7–30	4
Oct. 8–30	4
Nov. 10–19	2
Dec. 4–29	2

The correlation of solar constant departures with atmospheric transmission coefficient departures is $r = -0.305 \pm 0.097$, for the 99 usable values of 1925.

From the expressions derived above; i. e.—

$$\frac{\delta y_o}{y_o}=4.75\frac{\delta a}{a}\ \text{per hour}$$

and

$$\frac{\delta a'}{a_{av}}=-2.18\ \frac{\delta a}{a}\ \text{per hour}$$

we see that if the negative correlation of solar constant with atmospheric transmission is due to changing transmission, then the ratio of a percent change in solar constant to percent change in atmospheric transmission should be—

$$\frac{-4.75}{2.18}=-2.18$$

It is not possible to calculate an accurate value of the ratio from the data, but it is possible to set limits and see if the theoretical value falls between. If we assume no sources of variation of solar constant other than that due to changing atmospheric transmission, then we get an upper limit to the ratio, of percent change of solar constant to percent change of transmission. This upper limit is merely the inverse of the regression coefficient of atmospheric transmission on solar constant, and is 3.45. The lower limit is the regression coefficient of solar constant on atmospheric transmission, and assumes that there are no variations of transmission not tending to affect the solar constant. The value of this lower limit is 0.321. It is seen that the theoretical value 2.18 lies well in the region allowed by the rather wide limits.

If it is allowed that the mechanism cited above, changing atmospheric transmission affecting both solar constant and recorded coefficient, is the only and true mechanism causing the correlation of solar constant with transmission coefficient, then we can assume that 2.18 is the real ratio of percent change in solar constant corresponding to a percent change in transmission coefficient. Knowing this, we can express the variances of the solar constant and transmission coefficient in comparable numbers, the percent variance of the transmission coefficient being multiplied by $(2.18)^2$ to make it comparable to that of the solar constant.

Now if the variance of the two quantities being correlated are expressed in comparable units, the correlation coefficient is the ratio of the variance that is common to both quantities to the geometric mean of the variances of the two quantities. If—

V_c=the variance common to solar constant and transmission, and

V_s=variance of solar constant, and

V_t=variance of atomspheric transmission coefficient in comparable units, then—

$$r=\frac{V_c}{\sqrt{V_s.V_t}}$$

or

$$\frac{V_c}{V_s}=r\sqrt{\frac{V_t}{V_s}}$$

That is, the fraction of the variance of the solar constant that is associated with changing atmospheric transmission is equal to the correlation coefficient times the square root of the ratio of the variance of the transmission to the variance of the solar constant, expressed in comparable units.

When this is done, we get—

$$\frac{V_c}{V_s}=0.631$$

This is to be interpreted as meaning that 63 percent of the variance, or cause of deviation of the long method solar constant values from their monthly means is due to changing atmospheric transmission. Our estimate of the square of the percent standard deviation of solar constant from monthly means is—

$$\sigma^2=\frac{\Sigma(\text{deviations})^2}{99-12}=0.2938$$

63 percent of this is 0.1854, or the standard error of solar constant due to changing atmospheric transmission is $\sqrt{0.1854}$ or 0.4305 percent.

Of the three estimates of the standard error of a long method solar constant determination due to changing

tively absorbs radiation, aside from water vapor, is not taken care of. Ozone in particular will cause variation in transmission throughout a wide spectral region, and its effect will not be detected by either a measure of water vapor or of sky brilliance. In fact, the sky will tend to be less brilliant when there is more selective absorption, and decreasing sky brilliance is taken to be an indication of less absorption. Furthermore, it does not seem reasonable that all the factors determining atmospheric absorption at one wave length should be the same that determine the absorption for another wave length. As Dobson says (Proc. Roy. Soc. *A 104*, 252, (1923)), "One weak point seems to be that the values of $a\lambda$ for all different wave lengths are obtained from one observation on white light, while it is not necessary that the transparencies of light of all wave lengths should vary alike from day to day." In fact, Dobson got no relation between a measure of the ratio of sun brightness to sky brightness based on white light and the atmospheric transmission coefficient for $\lambda = 0.32\mu$ in the extreme ultraviolet.

Some indication of the fallacy of calculating all transmission coefficients from a measurement on white light is to be obtained from Abbot's published data. The experimentally determined atmospheric transmission coefficients for 10 different wave lengths are given for each day for which a long-method solar constant was derived, in table 28, page 169, volume 5. Let us take the ratio of the sum of the coefficients for the 5 longest wave lengths to the sum for the 5 shortest wave lengths. This ratio will vary in a somewhat regular manner with the general atmospheric transmission coefficient, which we will take as proportional to the sum of the individual transmission coefficients. If we call the above ratio R and the sum S, then the deviations of R from a regression line on S will be a measure of the variation of coefficients of short wave length that is independent of the variation of coefficients of long wave lengths. When these deviations are correlated with short-method solar constants for the corresponding days, a coefficient of -0.424 is obtained, for the 114 available days of 1928 to 1930 at Montezuma. The standard error of this coefficient is 0.077. This shows definitely the error in assuming that the coefficients for all wave lengths vary alike.

Abbot himself says, in volume 4, page 171, "The principal source of error in the short method is the uncertainty of the representation of the atmospheric transmission coefficients by the function-transmission plots for the numerous wave lengths." These plots are given for Calama at air masses 2 and 3, on pages 82 and 83 of volume 4. It is to be seen that there is quite a scatter in the points. The only quantitative measure of the scatter is given on page 171. The mean deviations of the points from the curve are given for curve 22 ($\lambda = 0.62\mu$), air mass 2, the deviations being grouped by function values. The average deviation of the 58 long-method determinations of a from the curve is 0.6145 percent. This wave length is in the middle of the spectrum, and the spread of points is fairly representative. If the deviations are normally distributed, the standard deviation will be 1.253 times this, or 0.77 percent.

This standard error of 0.77 percent in the determination of a from the values of the function is mainly due to three causes. They are the accidental error of determination of a by the long method, the error in the determined value of a due to changing atmospheric transmission, and the inability of the function to represent the true atmospheric transmission. We can calculate approximate values for the first two causes and attribute the remainder of the

error to the inability of the function to represent the true atmospheric transmission.

In the section on bolometry errors, estimates of the standard error of determination of a were given. A value of 0.347 percent was obtained from the standard error of the ratio of two sets of atmospheric transmission coefficients in 1919. This was thought to be low, as it does not include errors common to the atmospheric transmission coefficients of nearby wave lengths. For 1930, when errors should be more independent, a value of 0.455 percent was obtained.

For August 1, 1919, the standard error in the longer bolographic ordinates was about 0.73 percent, while for September 20, 1914, at Mount Wilson, it was 0.75 percent. The standard error in a least squares determination of a will be this divided by $\sqrt{\Sigma(m-\overline{m})^2}$ for the day. (See section on bolometry.) This factor is about $\sqrt{7}$ or 2.6. Taking 0.75 percent as the error in the bolographic ordinates, we get about 0.3 percent as the error in a for a "good" day. Thus we have three estimates of the standard error of determination of the atmospheric transmission coefficient, 0.3 percent, 0.347 percent, 0.455 percent.

For the error in a due to changing atmospheric transmission we find from the section on changing atmospheric transmission that the standard error in a due to this is $\frac{1}{2.18}$ times the error in E, the solar constant. The three estimates of this solar constant error were 0.10 percent, 0.31 percent, and 0.43 percent, with the latter most likely to be right. The corresponding errors in a due to changing atmospheric transmission, are 0.046 percent, 0.142 percent, and 0.197 percent.

If we take the largest estimated errors for a, we get

$$\sigma = \sqrt{(0.77)^2 - (0.455)^2 - (0.197)^2} = 0.589 \text{ percent}$$

This can be taken to mean that the standard error of determination of the atmospheric transmission coefficient from the function value is $\sigma_a = 0.589$ percent. This value is not to be regarded as very accurate, but it is difficult to see how it can be lowered much with the data given. If either of the other components of the 0.77 percent deviation of a from the plots is increased in order to reduce the estimated error of determination of a, the estimated standard error of a long-method solar constant determination must be increased. As it is already large enough to cover the full variation of long-method solar constant, this is not reasonable. Corresponding to this error in a at air mass 2, there will be twice the error in the extrapolated ordinate, for we get $\frac{\delta y_0}{y_0} = m\frac{\delta a}{a}$ when we differentiate $\log y = m \log a + \log y_0$. That is, the standard error in y_0 will be 1.178 percent. When summed up over all 38 wave lengths, this error will be reduced somewhat, but not by $\frac{1}{\sqrt{38}}$. For when the function is unable to give an estimate of the atmospheric transmission at one wave length it is likely to fail in many others near it. I doubt very much if the error could be cut in half by the summation over all wave lengths. If it were, it would represent an error of 0.59 percent in the value of the solar constant. And as atmospheric conditions will not vary much over a morning, this error is not much reduced by taking several observations during the morning. It seems to me reasonable to expect a standard error of about 0.5 percent in solar constant between daily values, due to the inability of the function to give exact values of the atmospheric absorption.

In the early days of short method calculations Abbot published the various values of the solar constant obtained during the individual days. These values lend themselves to an analysis of variance that is interesting in this connection. Data for the 4 months, August to November, 1920, for Montezuma, published in the Monthly Weather Review, 48, 665 and 772, (1920), were analysed. One hundred thirty-six observations are given for 59 days. The standard error of a solar constant determination as estimated from deviations from the daily means was calculated to be 0.388 percent. The standard error as calculated from the general 4-month mean is 0.667 percent. The standard error between daily means produced by causes not effective in producing the dispersion of values in a single day is the square root of the difference of the squares of these, or 0.542 percent. This value is not significantly different from the very rough estimate,

mass is about 10 percent, and as the function varies by a factor of more than 10, its addition can not be expected to yield more than "slightly better" curves. Therefore the estimate of a standard error of 0.5 percent is still as good an estimate as can be obtained from the published data.

G. EVIDENCE OF ATMOSPHERIC EFFECTS

Many writers have pointed out evidence that the solar constant values are associated with atmospheric conditions. As regards the long method values, it is perhaps sufficient to quote from the conclusions of H. Knox-Shaw, who made measurements from about 1913 to 1921 at the Helwan observatory, in Egypt. He said in 1915: "The high correlation existing between the computed values of the solar constant and the transmission coefficient indicates that the variation in the former is not real * * *."
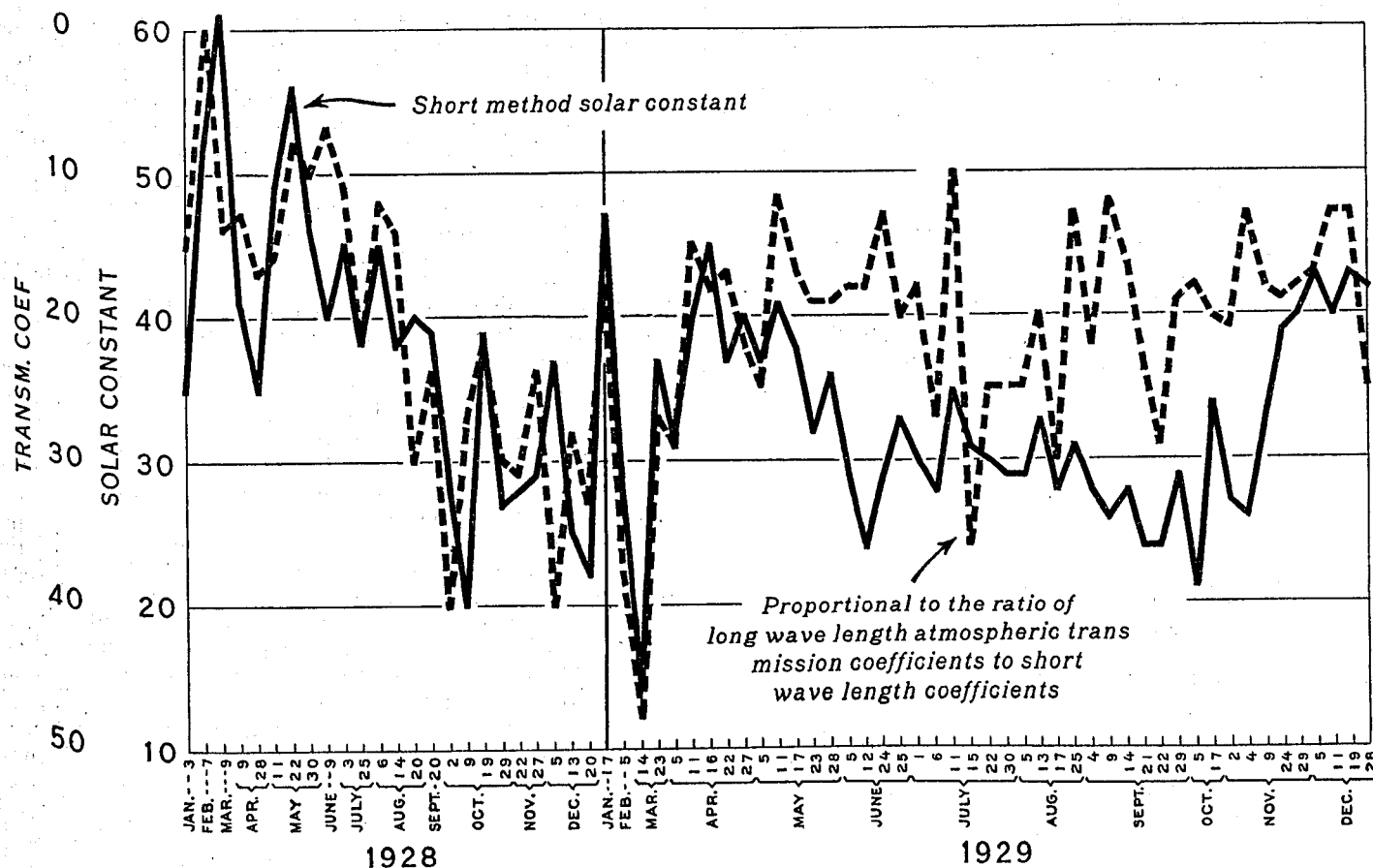
FIGURE 8.—Dependence of short-method solar constant values on atmospheric conditions.

0.5 percent, of the standard error to be expected from the inability of the function to give a valid estimate of atmospheric transparency.

The above calculations apply to the function used in 1920. In 1927 Abbot writes, (Beiträge zur Geophysik, 16, 344, (1927)): "We are substituting a modified function, which seems to yield slightly better curves." The new function is—

$$F = \frac{Pyrn. \times Area \; \psi}{Pyrh.}$$

Pyrn. is the pyranometer reading, used before. The area ψ is the area in mm² of one of the water vapor bands, and is equivalent to the old $\frac{psc}{\rho}$. The only new factor is the Pyrh. which is the pyrheliometer reading. As the extreme range of pyrheliometer readings at a given air

In 1921 he said:

Thus it seems fairly certain that at Helwan and probably at most other stations—correlation coefficients similar to those at Helwan have been found at Washington, Bassour, Arequipa, Calama and Mount Wilson—there is on fine days a progressive change in atmospheric transmission throughout the morning, which makes it impossible to determine the exact value of the solar constant on any particular day, or to detect variations in it, by observations made through different air masses and consequently spread over a period of 2 or more hours. * * * Such observations will be discontinued at Helwan * * * It is not claimed that the solar constant does not vary, but that its variations are masked by changes in atmospheric transparency, when observations are made through different air masses.

Various evidences of atmospheric control of short method solar constant values have also been put forth. W. E. Bernheimer (Problene der Astronomie, Festschrift fur Seeliger, p. 452, 1924) finds in 1921 a significant positive correlation between Montezuma short method

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solar constants and the amount of "precipitable water." In 1922 there is a significant negative correlation with atmospheric transmission coefficients. These correlations are calculated from daily values for separate groups of a few months and probably show a real relationship, independent of any yearly period in atmospheric factors possibly associated with an 11-month solar constant periodicity.

C. Dorno, (Monthly Weather Review *53*, 519, 1925) emphasizes a volcanic eruption in South America occurring on December 15, 1921. The implication is that the solar constant drop of 1922 might be associated with this volcanic eruption. The eruption of Katmai in 1912 was known to have caused a similar drop.

The largest drop in solar constant since 1922 was that of 1928. Figure 8 shows in the full line the Montezuma short method solar constant values of 1928 and part of 1929 for days on which there was a long method determination. In the dashed line are plotted values proportional to the ratio of atmospheric transmission coefficients of long wave length to those of short wave length, as determined during a long method observation. The inference
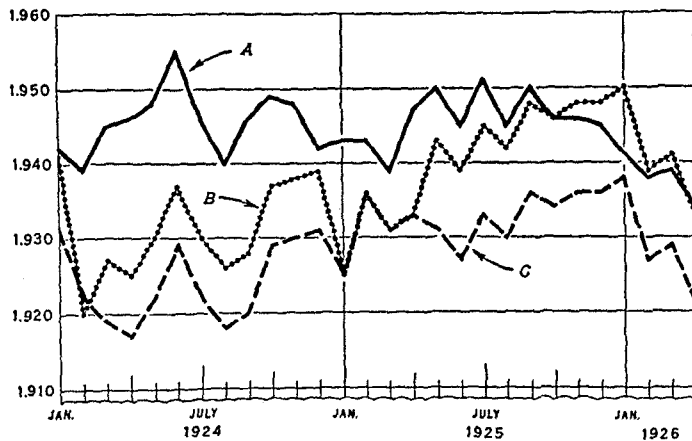

FIGURE 9.—Montezuma monthly solar constant values by various methods of reduction of same data. A. Final recalculated values, Annals, volume 5, page 258. B. First calculated values, corrected for selected pyrheliometry and watch eccentricities. C. First calculated values, uncorrected, Smith. Miscellaneous Collection, volume 80, No. 2, page 6.

that this drop in solar constant was associated with atmospheric effects is obvious.

These ratios, R, were taken as the sum of the atmospheric transmission coefficients for the 5 longest wave lengths divided by the sum for the 5 shortest wave lengths, given in table 28, volume 5. Let S be the sum of all 10 transmission coefficients, and E be the short method solar constant values for the days available. Let r_{ER} be the correlation between the solar constants and the ratios, and $r_{E.RS}$ be the multiple correlation of the solar constant with the ratios and the sums. The following table gives the values of these coefficients, together with the standard deviation of solar constant values and the number of observations, for each of the years 1928, 1929, and 1930, at Montezuma, and for all 3 years combined.

Year	r_{ER}	$r_{E.RS}$	σ_E	n
1928	−0.7056	0.7178	0.5377%	24
1929	−.4453	.4453	.3597%	45
1930	−.3546	.4275	.3240%	45
1928–30	−.5086	.5161	.4447%	114

The high correlation of solar constant values with one terrestrial condition is shown here. It should be noticed that in years when the scatter of solar constant values

was highest, the correlation with terrestrial phenomena is also highest.

H. A FEW KNOWN ADJUSTMENTS OF SCALE AND DISCONTINUITIES IN THE SERIES OF VALUES

Only the preferred monthly means from 1920 on will be considered. Until 1926, the values given depend largely on the values obtained at Calama and Montezuma, in Chile, with a few modifications from Harqua Hala values.

From January to August, 1920, the Chilean station was at Calama, Chile. In August it was put on higher ground, at Montezuma, and the method of calculation of short method values was somewhat changed. (Monthly Weather Review *48*, 540, 1920.) Therefore there is a slight change in method at this time.

In April 1924, it was noticed that the pyrheliometers in daily use at Montezuma were dusty (vol. 5, p. 86). After dusting, their readings were raised by 2.8 percent. The former director was then contacted, and remembered that about December 15, 1922, the room had been swept out. Some tests by selected pyrheliometry of a few days are given to show that the dust accumulated all at once on December 15, 1922. These tests seem inconclusive to me. Furthermore, the scale correction of 2.8 percent was determined by means of comparisons with the relatively inaccurate pyranometer. By reason of the uncertainties of this correction and the date of collection of dust, I think it impossible to deny that changes of scale might have taken place in December 1922 and in April 1924, and that in the meantime gradual changes might have taken place.

In 1927 a new procedure for the short method calculation was introduced. The values at Montezuma from 1924 on were recalculated by the new method, and the new values adopted. Up till 1924, the old method values were retained. Thus January 1924, marks a sharp discontinuity in the method of calculation. Figure 9 shows the solar constant values obtained at Montezuma for 1924–26, by the new and adopted method, and by the old method, the standard now up to 1924. For 1924, the first year that the new method was adopted, the average difference between new and old method values was 0.014 calorie, or 0.72 percent, the new values being higher by this amount. These results are taken from volume 5, table 40, page 258, and from the S. M. C. volume 80, No. 2, page 6. We can assume that there was a scale change of about 0.72 percent in January 1924.

From 1926 on, the preferred monthly means were derived from three stations, with the weights given: Montezuma (2); Table Mountain (1); Bassour (1), with 2 months at Montezuma and 1 at Bassour omitted. These stations are not independent, however, as scale adjustments took place. In 1927, at Table Mountain, dust had collected, indicating a 1.9 percent error. The corrections for the preceding period are necessarily somewhat uncertain (vol. 5, p. 139).

A pyranometer error occurred at Table Mountain, from August 1927 to September 1928 (vol. 5, p. 92). When finally corrected for, the intervening values were adjusted by selected pyrheliometry. However, a residual error in September 1928 was corrected for by a direct adjustment (lowering) of about ½ percent to the Montezuma scale. It is significant that this is the time of the 1928 fall of solar values, so the fall depends upon one station only. At the time of the rise of solar constant values in November 1929, the Table Mountain scale was raised by 0.4 percent (vol. 5, p. 251). The reason given is that the pyrheliometers were thought to be pointing unfavorably.

Another interruption in continuity of values occurred in 1932. In the Quarterly Journal of the Royal Meteorological Society, volume 60, page 73 (1934), Abbot says, "The volcanic eruption in southern Chile interrupted the Montezuma series. From May 1932, the values depend entirely on observations at Table Mountain, Calif., and are of less weight. New apparatus and new methods are being introduced at the Montezuma station. When completely discussed, about January 1, 1934, the Montezuma values from about August 1932, will be available. Until further discussion in connection with Montezuma values, the results here given are to be regarded as provisional."

IV. SUMMARY

For a single pyrheliometer reading, a standard error of 0.37 percent is reasonable to take, corresponding to a probable error of 0.25 percent. This agrees with the only actual test for which complete data are given. It would correspond to a probable error of 0.17 seconds in determining the time of a reading, and a probable error of reading the temperature of 0.004° C., or 1/25 of the nearest scale division. In addition, slight changes of scale of as much as 0.2 percent or more can well have occurred frequently, due to accumulation of dust or faulty pointing of the instrument.

The errors of bolometry can best be estimated from actual data. From the 2 days' data available, the best estimate of a standard error of the bolographic reduction for an excellent day is about 0.30 percent.

The instrumental error of a long method reduction will be a combination of one bolometric error with the error of the average of two pyrheliometer readings. Thus the standard instrumental long method error will be—

$$\sqrt{(0.30)^2+\frac{(0.37)^2}{2}}=0.40 \text{ percent}$$

The error of a long method determination due to changing atmospheric transparency is more difficult to estimate, but is probably in the neighborhood of 0.43 percent, as estimated from the data of 1925. Combined with the standard instrumental error, a total standard error of a long method determination of about 0.59 percent is obtained. This is just equal to the standard deviation of the 1925 long method solar constants from their mean value.

The equality of the errors and fluctuations of solar constant indicates that any real variations of the sun are completely masked by errors, instrumental and atmospheric, of the long method.

The bolometry error for a short method is the error of determination of the area of water vapor bands. The standard error of such a determination depends upon the amount of water vapor, but according to Abbot will range around 0.13 to 0.31 percent, with a mean of 0.22 percent. The main instrumental error of a short method determination will be due to a combination of this with the error of the average of two pyrheliometry readings. If n short method observations are taken during the day, the standard instrumental error of a day's short method result will be $\sqrt{\frac{(0.22)^2}{n}+\frac{(0.37)^2}{2n}}$. If there are five observations during the day, this will amount to a standard instrumental error of 0.15 percent for a day's mean value. For a single observation, the standard instrumental error will be about 0.34 percent.

Atmospheric errors in the short method are caused by fluctuations in the amounts of the absorbing constituents in the atmosphere. Consequently atmospheric errors will take effect between daily values, rather than between values of the same day, and the taking of five observations during the day reduces the atmospheric errors very little. An estimate of these is not claimed to be accurate, as very little data are available from which they can be calculated. However, a standard error of about 0.5 percent between daily values was considered the best estimate of atmospheric errors.

An analysis of 4 months of 1920 shows the standard deviation of short method solar constant values from the daily mean of 0.39 percent, which should be compared to the 0.34 percent error estimated for two of the instrumental sources. The extra variation between daily means not due to the same causes as the variation within days had a standard deviation of 0.54 percent, which is to be compared to the 0.5 percent estimate of atmospheric error.

The total estimate of error of a daily value of five observations would be $\sigma=\sqrt{(0.15)^2+(0.5)^2}=0.56$ percent. This is enough to account for the fluctuations of short method solar constant values of any recent year.

COMPARISON OF CONTEMPORANEOUS MEASUREMENTS OF THE SOLAR-CONSTANT

By Larry F. Page

For 35 years, more or less regular observations of solar radiation have been made by the Smithsonian Institution. Due to subsequent improvements in methods and instruments, the solar-constant values obtained before 1920 are not now considered reliable. Since that time, however, there have always been at least two observing stations in operation whenever atmospheric conditions permitted. Enough data have accumulated to make possible statistical comparisons between values of the solar-constant determined from observations at different localities or at the same place by different methods. Some such comparisons have previously been made, by means of correlations calculated or estimated (1) and by computing measures of the absolute difference between two series of observations (2).

The question of actual variation in the solar-constant was discussed in detail in 1925. On the basis of: (a) high correlation between solar-constant and atmospheric transmission, (b) low correlation between Abbot's solar-constant and that measured by Kimball, (c) an annual period in pyranometer readings, and (d) the small dispersion of daily values, Marvin (3) concluded that the reality of variation of the sun had not been shown, and further data were necessary to determine it.

Kimball (1) calculated correlation coefficients between Montezuma and Harqua Hala daily short-method values and showed that the variation common to both was less than the secular variation as shown by monthly means.

In rejoinder, Abbot has said (a) that there is no real annual period, (b) that the average differences between contemporaneous observations of different stations were very small, thus showing that the values actually refer to variations in the sun, and (c) that fluctuations of the solar-constant are related to the weather and therefore must be real.

After a complete analysis of the methods used, H. G. MacPherson elsewhere in this volume shows that the errors of observation and reduction are enough to account for the day-to-day variation reported. Proof of the reality of the variations depends now almost wholly on agreement between values obtained at different stations.

The data themselves offer some difficulties because of changes in scale and adjustments which have been made. A large secular change took place in 1921, which must have been due at least in part to volcanic dust from an eruption in South America (4). In April 1924, it was noticed that Montezuma pyrheliometers were dusty (2b, p. 86). It was recalled that the room had been swept out about December 15, 1922. Some tests by selected pyrheliometry of a very few days were made which seemed to indicate that the dust had all accumulated on this date. A scale correction of 2.8 percent, determined by the relatively inaccurate pyranometer, was applied to all the intervening values. A new method of calculating the short-method values was applied to all data

from 1923 on, making another discontinuity in the data. In addition to other adjustments, "the whole series of solar-constant values from 1924 to the present time (1930) was reduced to a consistent and definite system" (2b, p. 122). A pyranometer error, due to dust, affected Table Mountain short-method values from August 1927 to September 1928 (2b, p. 92). The values were adjusted by selective pyrheliometry. However, a residual error was corrected by direct adjustment of scale by comparison with Montezuma values. In November 1929, the Table Mountain values were too low and were raised 0.4 percent because it was thought that the pyrheliometers had been "pointing unfavorably" (2b, p. 251).

In addition to these and other changes, the values at Montezuma are "corrected" by a factor to make the scale agree with that of the old observations at Mount Wilson (2b, p. 123), and the scales of the other stations have been brought into agreement with Montezuma (2b, p. 278). For some other adjustments, see (2b, p. 121). It is of importance to note that fewer adjustments have been made to long-method observations.

The records used, published in volume V of the Annals, are as follows: Montezuma, Chile, August 1920–December 1930; Table Mountain, Calif., December 1925–December 1930; Mount Brukkaros, Southwest Africa, December 1926–December 1930; and Harqua Hala, Ariz. (long method only) October 1920–June 1925. Mount St. Katherine, Egypt, and Montezuma values for June to November 1934 were published in reference (5).

Let us consider first the evidence of long-method values. The long method is the fundamental one developed by Langley. The only errors in it which are not also in the short method are due to the fact that it requires observations covering a period of about 2½ hours. Changes in the sun or in the atmosphere during this time will affect the result. Certain other errors are present in the short method which do not affect long-method reductions. Correlations between long-method observations on the same days are:

Stations	Interval	n	r
Montezuma–Table Mountain	Dec. 1925–Dec. 1930	49	0.12
Montezuma–Mount Brukkaros	Dec. 1926–Dec. 1930	20	-.06
Montezuma–Harqua Hala	Oct. 1920–May 1925	34	.06
Table Mountain–Mount Brukkaros	Dec. 1926–Dec. 1930	53	.15

These are obviously negligible.

Short-method observations are subject to wider secular variations, some of which, at least, may be due to changes of scale or adjustments. Since many daily values are available from Montezuma and Table Mountain, these were taken as departures from the monthly means. Correlations between Montezuma and Table Mountain on the same days are given below:

Month	n	r	Month	n	r
December, 1925	9	-0.54	1928—Continued		
			June	24	-0.01
January, 1926			July	23	.49
February	17	-.04	August	15	.34
March	4	.88	September	13	.12
April	12	-.41	October	23	-.27
May	13	-.19	November	18	-.38
June	10	.23	December	19	-.15
July	14	.76			
August	24	.26	1929		
September	13	-.12	January	7	.10
October	22	.19	February	15	.07
November	29	.25	March	15	-.18
December	15	.45	April	18	-.00
	13	.16	May	18	-.01
			June	10	.09
January, 1927			July	18	-.17
February	9	-.64	August	8	-.59
March	4	.50	September	23	.23
April	13	-.11	October	11	-.30
May	21	.13	November	25	.31
June	24	-.35	December	19	.12
July	18	-.13			
August	17	.01	1930		
September	18	.25	January	6	-.07
October	24	-.21	February	13	-.35
November	22	-.14	March	12	.16
December	18	.21	April	12	-.29
	14	.08	May	7	-.11
			June	21	.20
January, 1928			July	22	.51
February			August	16	.13
March	14	.20	September	21	-.15
April	17	-.27	October	26	-.25
May	17	.02	November	16	-.28
	18	-.26			
	9	.29	Entire period	966	.01

Out of 60 values, 30 are positive, 1 is zero, and 29 are negative. The correlation for all days taken as departures from the respective monthly averages is 0.01, for 966 pairs.

Only 6 months' observations have been published for Mount St. Katherine, from June–November 1934, in "Mount Saint Katherine, An Excellent Solar-Radiation Station," (5). On page 2 of this reference, Abbot, after calculating the average difference between observations at Mount St. Katherine and Montezuma, states, "Hence, it is with unusual satisfaction that I am able to report the close agreement between the results obtained at Mount St. Katherine, our new station in Egypt, and those obtained on the same days at Montezuma in Chile," and on page 5, "The close accord shown by these two remote and contrasting stations cannot but encourage the belief that the observations of the variability of the sun hitherto reported from Montezuma are very close to the truth."

The correlation coefficient between observations at the 2 stations on the same days is 0.03, with 99 pairs. However, in the reference just cited, Abbot calls attention to the fact that the observations during November were not quite as reliable as those for the other months. Omitting these, the correlation is −0.04, with 88 pairs. In both cases only "satisfactory" or "nearly satisfactory" values were used.

Turning now to the monthly averages of short-method determinations, the correlations are:

Stations	Interval [1]	n	r
Montezuma–Table Mountain	December 1925–December 1930	60	0.65
Montezuma–Mount Brukkaros	December 1926–December 1930	48	.05
Table Mountain–Mount Brukkaros	December 1926–December 1930	48	.28

[1] May 1928 was omitted because no reliable values were obtained at Table Mountain in that month.

The Montezuma–Mount Brukkaros correlation is practically zero. That for Table Mountain and Mount Brukkaros may need some explanation. If monthly values are taken as departures from the annual means instead of from the means of the whole period, this becomes 0.07. This leaves the only agreement of any sort in these two records between annual means, which certainly must have slight weight as evidence of real agreement in view of the many adjustments of scale. Pyrheliometers were injured while being taken to Mount Brukkaros and "by extensive comparisons with Montezuma, scale corrections were fixed" (Annals, vol. V, p. 243). Several different scale corrections were made for different periods.

We come now to the correlation coefficient of 0.65 between monthly mean departures from the average for the entire period, at Montezuma and Table Mountain. Taking these as departures from annual means, the coefficient is still 0.56. Correlations of monthly values in the individual years are as follows: 1926, $r=0.12$; 1927, $r=0.30$; 1928, $r=0.92$; 1929, $r=0.56$, and 1930, $r=-0.04$. It may be noted that because of the large fluctuations in 1928, the agreement in that year affected the total correlation more than it would have otherwise—that is, 1928 is weighted more than the other 4 years. (The standard deviations are: Montezuma, 1926, 1927, 1929, 1930, $\sigma=3.88$; 1928, $\sigma=8.26$; Table Mountain, 1926, 1927, 1929, 1930, $\sigma=3.67$; 1928, $\sigma=8.25$.)

If the real relationship between observations is that indicated by a correlation of 0.92 or even 0.56, there is no explanation for the coefficient of −0.04 in 1930. But, if the real relationship is zero, or nearly so, there is good reason to expect high spurious correlations in both 1928 and 1929. It may be recalled that, in 1928, errors developed at Table Mountain which eventually required adjustment on a basis of Montezuma values. In 1929, the two stations were again in disagreement and the Table Mountain scale was corrected because "it was suspected that the pyrheliometers * * * might be pointing slightly unfavorably."

A further test may be made of the agreement of observations. On many days the solar-constant has been determined at the same station by long and short methods. These are not independent observations, so agreement between the methods would not indicate reality of solar variation. Disagreement, however, may throw some light on the errors of measurement and reduction. Except for the possibility of real solar changes occurring between measurements on a given day, only errors in one method or in both will reduce their correlation coefficient from unity. Below are the correlations between long- and short-method solar-constants on the same days at the same station.

	Montezuma		Table Mountain	
	n	r	n	r
1920	50	0.62		
1921	24	.27		
1922	31	.86		
1923	73	.42		
1924	51	-.01		
1925	87	.16		
1926	18	.27	62	0.23
1927	28	-.25	47	.09
1928	15	.51	48	.23
1929	37	.04	60	.25
1930	42	.14	61	.01
Period	456	.19	278	.25
1920-22	105	.63		
1923-30	351	.07		

The correlations of Montezuma and Table Mountain long- and short-method values for all years combined, are 0.19 and 0.25 respectively, indicating that only about 3.6 and 6.2 percent of the variation is common to the two

methods. Hence, we may conclude either (*a*) that there was practically no variation in the sun, or (*b*) that one or both methods almost completely fail to record solar changes. It is probably significant that the correlation at Montezuma for the first 3 years was 0.63 and that it fell to 0.07 during the period 1923–30. The use of the abbreviated short method was begun January 1, 1923, and continued to the present. This method is described on page 111, of volume V of the Annals:

Our new abbreviation consists in eliminating this * * * computation (the logarithmic computation involved in determining the form and area of the bolograph as it would be outside the atmosphere) by computing certain tables once and for all. This may be done because exact knowledge of the form of the solar-energy curve is not essential * * *. This statement may be confirmed by referring to a previous page, where the results of computing solar-constants with different optical transmission data are compared. By analogy it follows that only an inappreciable change of result by the short method would occur if, instead of using the ordinates of the smoothed energy curve actually observed on a given occasion, we should employ the average ordinates of a number of bolographs previously taken at the same air mass.

On the following page, this statement is made: "We have tested the new 'short method' against the earlier 'short method' for identical days and find as was expected, that the two give identical results." Nevertheless, Dr. Abbot recently announced (April 1937) that there is a fundamental error in this newer short method and that when data are re-reduced by the original method there may be changes of at least 50 percent in the daily solar-constant values. We have seen, however, that no correlation exists between contemporaneous long-method observations at different stations. Therefore, if a new reduction of short-method results is made which would bring them closer to the long-method values, no further agreement between stations could be expected.

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SOME STATISTICAL TESTS OF SOLAR CONSTANT—WEATHER RELATIONSHIPS

By Larry F. Page

Connection between variations in the solar constant and those in weather phenomena has been based principally on two theses—that periodicities in solar radiation are associated with those in weather data, and that certain patterns in temperature follow more or less marked changes in the solar constant. These will be considered separately.

It is fair to say that, if the cycles in solar radiation data are not significant, they are of no value in determining weather changes. The first part of this paper is, accordingly, restricted to a study of periodicities in observed values of the solar constant.

A statistical analysis of this sort means a study of what records of the variable are available, taken as a sample, as a basis for calculating the probability that this sample is, within certain limits, representative of the entire universe of similar data. When applied to time series, a forecast is implied. That is, the universe includes the future as well as the past. When we test, for instance, a periodicity to see whether it may be "real," we are actually testing whether the sample might be expected to be enough different from the universe that the amplitude and duration we find may be due to fluctuations in sampling. When we apply standard errors we make a number of assumptions, and, unless these assumptions are valid, the tests for significance have no meaning. The tests are based on mathematical conceptions and apply only if our data follow the premises of mathematical sampling.

One of the most important of these premises is that each observation must be independent of the preceding one. Monthly solar-constant data, as will be seen, do not satisfy this requirement. The correlation periodogram is based on the proper assumption that, if data are periodic, they tend to repeat after an interval equal to the length of the period. The coefficient of correlation between the series $x_0, x_1 \ldots x_{n-p}$ and itself at a given lag $x_p, x_{p+1} \ldots x_n$, is plotted against the length of interval, p. This method has been applied to monthly solar-constant values, (1) 1920–1934, inclusive; 180 values in all, with result shown at the top in figure 1. The curve of the periodogram shows a coefficient of 0.82 at a lag of 1 month. Each observation is, therefore, related to the preceding one, and to several before that. This lack of independence will be referred to as serial correlation. This means that a comparison of the correlations with their standard errors, either directly, or by applying Fisher's z transformation, is not valid. The problem of testing significance in such cases has not been completely solved. The solutions suggested fall in three general classes: (1) adjust the tests of significance, (2) eliminate the serial correlation—usually by taking differences, and (3) compare results with those in which a serial correlation has been imposed upon random data. Bartlett (2) has published an approximation for the distribution of correlations from samples in which a certain type of serial correlation is present similar to the type in these data. $Vr_{xy} = \frac{1}{n}\left(\frac{1+r_x r_y}{1-r_x r_y}\right)$, where r_x is the serial correlation in the x series, r_y the serial correlation in the y

series and V is the variance. This was applied in the following way. The correlations at lags 1 to 10 were omitted as being directly affected by the dependence of successive observations. The 90 remaining coefficients, at lags 11 to 100 were divided by their standard errors, as calculated from Bartlett's distribution. The distribution of these ratios is compared with a normal curve at the bottom in figure 1. A perfect fit could not be expected. There appear to be slightly more cases above twice the standard error than would be expected, but the differences are not significant.

I should like to digress a little here to point out what seems to me a fundamental error common in the use of

CORRELATION PERIODOGRAM, MONTHLY SOLAR CONSTANT
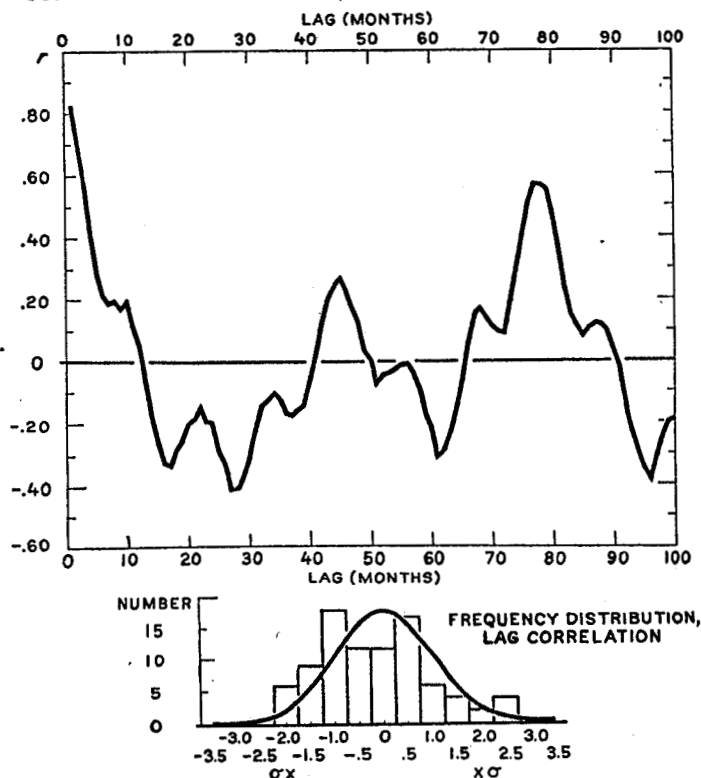
FIGURE 1.

the correlation periodogram. The ratios shown which were greater than +1.75 were all for correlations from lag 75 to lag 80. If we were willing to say, on the basis of comparison with the expected distribution, that the correlation at lag 77, which is the highest value found, is significant, would we be justified in saying that there is a periodicity of 77 months in the solar constant? It has been at least implied in the literature that, since we are dealing with 103 pairs of observations, the significance dependent on this many cases can be transferred into a feeling of confidence in the period as such. But, in fact, there are only two periods of this length and a small part of a third. As far as the periodicity itself is concerned, even if the correlation were perfect, there are

(121)

only 2 cases, not 103, and inferences must be made accordingly.

The second method suggested to overcome serial correlation is differencing. First differences were taken and the correlation periodogram method applied to them. If a periodicity exists, that is, if the data tend to repeat after a certain interval, the first differences will also repeat to the same extent. The serial correlation in the first differences is −0.19. The periodogram, shown in figure 2. is entirely different, with the highest value only 0.27 at a lag of ten months. One of the criteria in a correlation periodogram is that there shall be high coefficients at all multiples of an apparent period. It is obvious that this is not the case here. The distribution of the coefficients is approximately that expected by chance, only slightly larger frequencies at the extremes.

Finally, an attempt was made to take series in random order and to inject an amount of serial correlation ap-

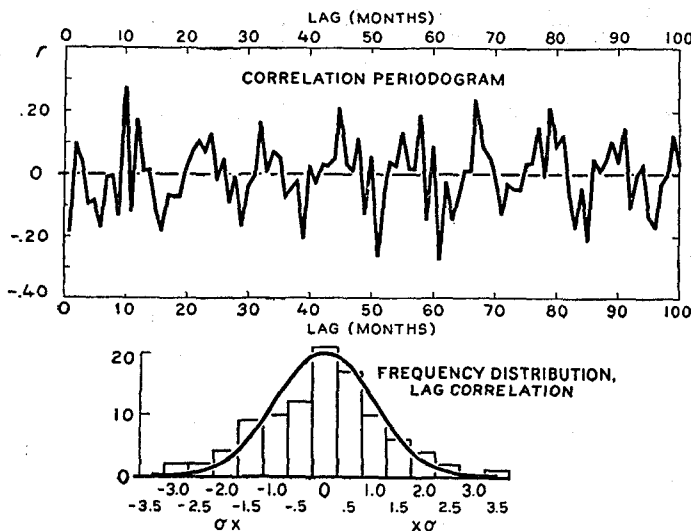FIRST DIFFERENCES,-MONTHLY SOLAR CONSTANT, 1920-34

FIGURE 2.

proximately the same as that in the solar constant series being studied. In order to retain as nearly as possible the same original frequency distribution, the 180 departures from the mean of the solar constant were used. They were put in random order with the aid of Tippett's numbers. Then, serial correlation was introduced by taking a simple moving average. The correlations shown in figure 3, at the first 12 lags are as close to the original as can be expected. In each case, the coefficient becomes negative at lag 13. There are at least minor peaks at 26, 52, and 81 months, approximately multiples of 26, indicating a "true" periodicity of 26 months. Or, if we are looking for high coefficients instead of regular peaks, there is a period of 99 months, with a lag correlation of +0.46, from 81 pairs. The standard error, as usually applied is +0.09, giving a ratio of 5.23 for r/σ_r. Such a ratio will occur, due to chance, only once in 10 million times. We can even afford to remember that it was picked as the largest out of 100 correlations and say that the chance probability is 1 in 100,000. However, using Bartlett's estimate of variance, the correlation of 0.46 is only 1.68 times its adjusted standard error, an event to be expected at least once in the hundred coefficients calculated. Since we know that there are no real periods in these random data, we expect approximately a chance distribution of the ratios of coefficient to adjusted standard error. Even the high peak at −0.5 is within the limits of sampling errors of 90 coefficients.

Thus it has been shown by taking into account the serial correlation, and applying three methods of overcoming errors due to it, that periodicities which appear in this sample of data cannot be expected to persist, and are therefore of no use in forecasting.[1]

There is, however, a more obvious test of the value of periodicities in forecasting the solar constant. Three
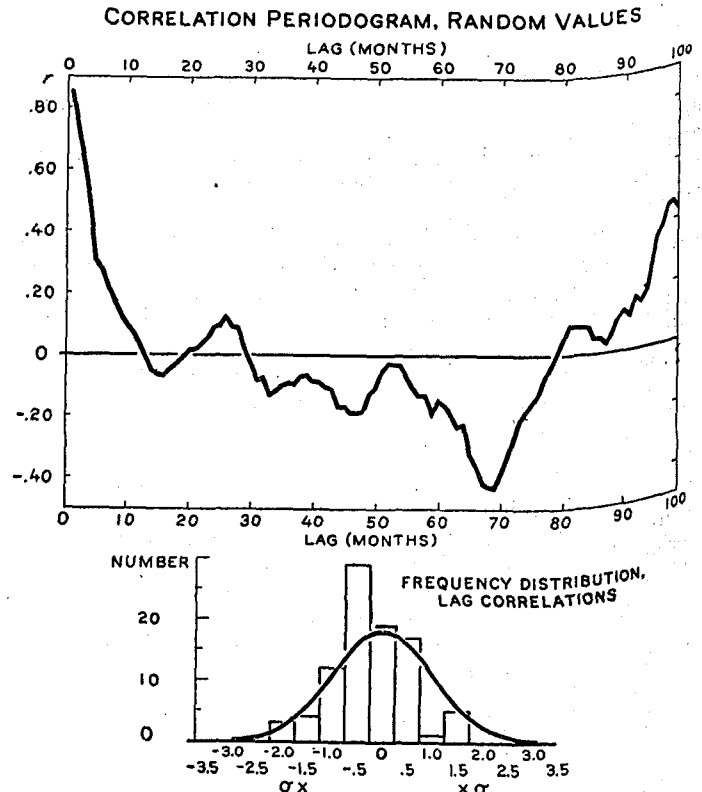

CORRELATION PERIODOGRAM, RANDOM VALUES

FIGURE 3.

predictions on this basis have been published, for 1931-32 (3), 1933-34 (4), and 1935-37 (1). Data for comparison were available from January 1931 to September 1936, inclusive. The correlation coefficient between predicted and observed values is 0.05. This measures only fluctuations about the means and shows that month-to-month

[1] This problem of serial correlation seems to have been overlooked in a recent paper by T. E. Sterne (Proc. Natl. Acad. Sci., 25, 559-564 (1939)) in which he gave the results of harmonic analysis of 10-day values of the solar constant. He finds probabilities by chance of the amplitude terms as low as 6×10⁻¹⁰, and finds 7 of 10 periods suggested by Abbot between 7 and 68 months all to have probabilities by chance of less than 3×10⁻⁴, giving a combined probability of 5×10⁻²⁷ for the occurrence of all of them. He concludes, "It is found that seven periods (9.75, 11, 21, 25, 39.5, 46, and 68 months) are of strongly suggested statistical significance and must in all probability be present either in the systematic errors of the 10-day means, in the solar radiation, or in both. Three [other] periods can reasonably be attributed to statistical fluctuations."

The probabilities mentioned above would, indeed, be small and would be almost certain indication of periodicity if the data followed the laws of mathematical sampling. However, it has been shown by Bartels (Terr. Mag. and Atmos. Elec., 40, 1-60 (1935)) that if data are correlated serially the expectancy is greater for longer periods than it is for shorter, i. e., that the simple expressions for the probability of harmonic terms is not valid. Random data which have been smoothed will give similar apparently significant amplitudes for periods of greater length and correspondingly smaller amplitudes for short periods than would be expected by chance, according to the usual tests. This does not depend on the smoothing method actually superimposing periodicities on the data, but on the fact that more of the variation of a smooth curve can be represented by long waves than by short ones.

Considering the 180 monthly values of the solar constant from 1920-34, complete harmonic analysis would give 90 periods independent of each other, ranging in length from 2 to 180 months. But, only 17 of these are longer than 10 months and the 15 between 10 and 60 months account for 76 percent of the total variation from the mean. Every possible independent period between 10 and 60 months in length would be statistically significant according to the tests based on uncorrelated data.

This may be treated in another way, as was done above for the correlation periodogram, by considering the first differences of the series. If there is, in fact, a true period of sinusoidal form, it should also appear in the differences with a phase change of 90°. If the "errors" imposed on it are random, then the average squared error of their differences will be twice the average squared original error, and allowance must be made for this. The 25-month period, which Sterne found to have a probability by chance of 7×10⁻¹¹, was treated in this way. In order to be significant, the average square due to the two degrees of freedom used by the sine curve must be significantly larger than that due to the remainder. The values found were 15.67 and 37.14, respectively. Dividing the latter by two, it is still larger than the average attributable to the sine curve, and this is hence found not to be significant.

variations at least were not forecast correctly. In order to test in another way the utility of the forecasts, the average difference between forecast and observed was compared with the average deviation from the general mean of the previous years, both being taken without regard to sign. The average error of forecasts is 9.25; the average error if the "normal" had been used as a forecast is 5.39.

Where, then, did these periodicities come from? The method used by Abbot to obtain their periods and amplitudes is so often used in meteorological data that it warrants some attention. The data are arranged in a table with the length of each row equal to the trial period. The columns are averaged and one-half the difference between the highest and lowest averages is taken as the amplitude. Trial periods having large amplitudes are considered real. The following table gives the period length and amplitude as determined by Abbot:

Months	7	8	9¾	11	21	25	34	39½	46	68	92	¹23
Amplitude	8	32	30	78	97	76	89	127	143	162	157	81

¹ Years.

It is evident that, in general, the longer the period the greater the amplitude, except in the case of the 23-year period. This nonsinusoidal "period" of 23 years, determined by 15 years of data, will not be considered. The tendency for longer periods to have larger amplitudes has been noted by some investigators of meteorological periodicities and been given as a sort of physical law. Let us look into the real reason for this. Considering the 180 monthly values of the solar-constant, we can determine the probable amplitude to be expected by chance for periods, say of 12 and 60 months. In the former, the data will be arranged in 12 columns of 15 items each. The standard error of the mean is $\frac{S.\ D.}{\sqrt{n}}$, where n is the number of rows or, in this case $\frac{8.65}{\sqrt{15}}=2.23$. Out of 12 means, we may expect a maximum amplitude of about 3.8 units. Now, with a 60-month period we have 60 columns of only 3 items each. The standard error of the mean of a column is $\frac{8.65}{\sqrt{3}}=4.99$. This standard error is not only larger, but we have 60 instead of 12 means from which to choose a high and a low value. The expected maximum amplitude, by chance, is about 12.3. It is no wonder that amplitudes determined in this way are greater for long periods than for short ones. It is due, however, to a fallacy in the method, not to a physical law.

TABLE 1

11-month period	Solar constant		Random data		11-month period	Solar constant		Random data	
	∅	$\frac{R}{s}$	∅	$\frac{R}{s}$		∅	$\frac{R}{s}$	∅	$\frac{R}{s}$
	°		°			°		°	
1	34	1.03	180	0.26	10	61	1.35	125	1.26
2	359	1.01	65	.15	11	341	.78	109	1.04
3	66	1.27	123	1.20	12	140	1.03	106	.70
4	272	.79	304	1.16	13	236	1.18	316	1.22
5	299	1.28	218	1.01	14	91	1.23	348	1.37
6	27	.45	80	.68	15	113	.99	331	1.17
7	69	1.23	249	1.04	16	103	1.04	119	.85
8	347	1.23	10	.62	Average		1.05		.94
9	52	.97	34	1.25					

There remains the question of change in phase. Since the 11-month period has a fairly high amplitude and since

there are 16 samples of it in the series, it was studied in some detail. Each 11 months was fitted to a sine curve of that length. The amplitudes and phases are given in table 1. Since there are large variations in the standard deviations for the different periods, amplitudes have been expressed as ratios of the standard deviation.

The phase of a sine curve fitted to 11 points will not differ greatly from that determined from the peak of smoothed values. There is no regular variation, nor consistent phase during an 11-year sunspot period, as has been suggested. The random series with serial correlation was subjected to the same treatment. Since we picked the best example of short period from the solar-constant data, it might have been more comparable to follow the same procedure with the random series. In order to avoid differences arising from the influence of serial correlation in short periods, however, 11 months was used in this case also. The averages of $\frac{R}{\sigma}$ are 1.05 and 0.94 for solar-constant and random data, respectively, not a significant difference.

TEMPERATURE PATTERNS

The balance of this paper will be devoted to a study of the asserted effect on the course of temperature of daily solar-constant changes. Publication of evidence in support of this hypothesis was begun in 1932 (5) and continued in 1936 (6). The latter has been used, since, if there are any essential differences, it may be assumed to be more reliable as representing more years of work on the subject. For the most part the study has been confined to 2 months, April and August, and to Washington, D. C., temperatures. These were not picked out as being either the best or worst examples.

Four criteria were set up by Abbot (6) for testing this hypothesis. They are perhaps sufficient for our purpose.

1. Since opposite causes generally produce opposite effects, whatever curves may represent the average courses of the temperature departures following rising solar sequences, they should be opposite, as the right hand is to the left, to the average curves representing the after effects of falling solar sequences.
2. Since greater causes generally produce larger effects, exceptionally wide ranges of solar variation should be associated with larger temperature ranges than the average of all the cases.
3. Since similar causes generally produce similar effects, the average results found in the years 1924 to 1930 should closely resemble in phases, though not necessarily in amplitudes, the average results found in the years 1931 to 1935.
4. Since the sun shines on the whole earth, temperature effects which fulfill criteria 1, 2, 3 should be found at all stations.

Abbot's general plan was as follows. Since single day's observations are subject to some error, (6) "It is only by a well-supported series of quite a number of successive days' values of high apparent reliability, trending steadily in a given direction, that we may be well assured that a real indication of solar change is before us." From plotted values, zero dates of rising and falling sequences of solar change were chosen. Temperature departures from normal at Washington, D. C., Helena, Mont., and St. Louis for 17 days beginning with the zero date were tabulated and averaged separately for each station for each calendar month for rising and for falling solar values. To test criterion 2, a case was given in which large and small solar variations were segregated and the resulting temperature sequences compared. Average results for 1924–30 and 1931–35 were separated and compared to test the continuity of the supposed relationship, as stated in criterion 3.

Since all conclusions are based on the determination of the zero dates of rising or falling solar-constant values, it is important to see how these were chosen in the 2 months studied. Volume V of the Annals of the Astrophysical Observatory gives daily values at Montezuma, Chile, from 1924 and 1925, and preferred solar-constants, "the most probable daily values" from 1926-30, inclusive. The office of the Astrophysical Observatory supplied April and August daily values from Montezuma from 1931-35, inclusive. Abbot's study was based on Monte-

DAILY SOLAR-CONSTANT VALUES SHOWING ZERO DATES OF RISING AND FALLING SEQUENCES AS GIVEN BY ABBOT, APRIL 1924-27
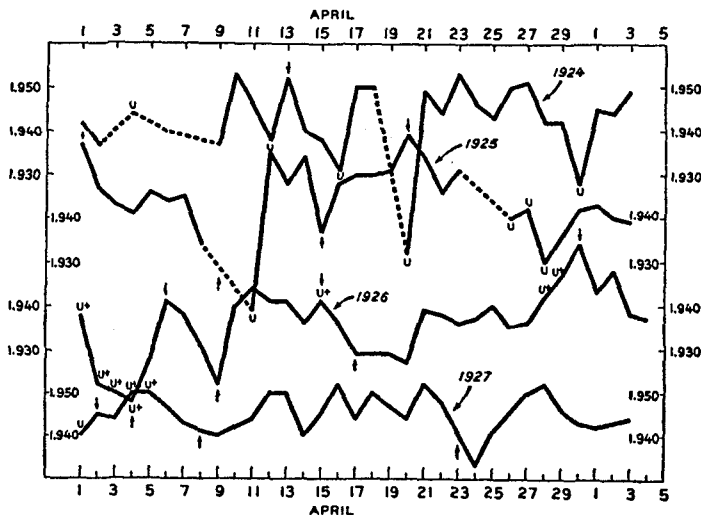
FIGURE 4.

DAILY SOLAR-CONSTANT VALUES SHOWING ZERO DATES OF RISING AND FALLING SEQUENCES AS GIVEN BY ABBOT, APRIL 1928-31
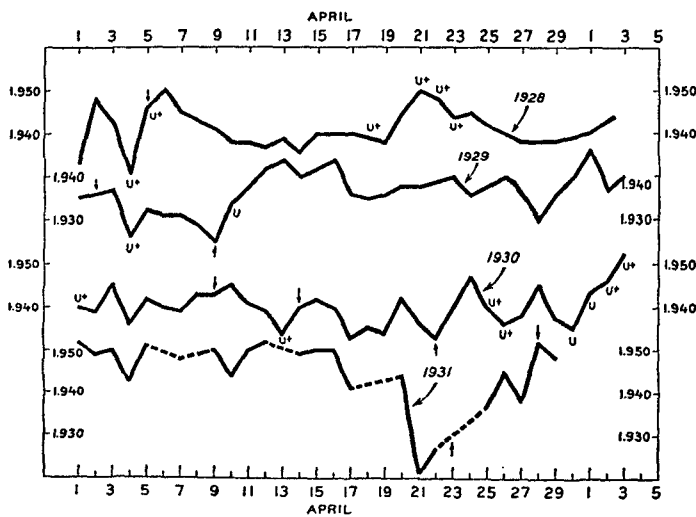
FIGURE 5.

zuma values alone, but I chose the preferred values where they were available on two considerations. (1) These were compiled by the Smithsonian Institution as "the best evidence of the four stations as to the real variation of the sun." (2) There should be only small differences, since (quoting from the Annals) "We have reduced the values from other stations to agree as closely as possible with the Montezuma scale. When a Montezuma value appearing to be normal in the march of values is available, we have given it greatly preponderating weight whatever other stations might indicate." These are given with

grades, S, $S-$, $U+$ and U, from satisfactory to unsatisfactory. $U+$ and U are indicated in figures 4-6; other values are S or $S-$. Where no value was recorded, the points are connected with a dashed line. Arrows, pointed upward or downward, show the dates of beginning of significant rises or falls respectively of solar radiation, as chosen by Abbot.

On April 9, 1925, an ascending sequence is supposed to begin. No values were published for either the 9th or 10th and the 11th is below the 8th. April 17, 1926, ascending sequence. Actually, the 18th and 19th are the same as the 17th and on the 20th, the value is lower. Other questionable cases are: April 2, 8, 23, 1927; April 5, 1928; April 2, 1929; April 9 and 14, 1930; April 23, 1931, and April 24, 1934.

These dates, given in (6) table I, have been checked by comparison with figure I of the same paper in which they are indicated graphically, and by reproducing the Wash-

DAILY SOLAR-CONSTANT VALUES SHOWING ZERO DATES OF RISING AND FALLING SEQUENCES AS GIVEN BY ABBOT, APRIL 1932-35
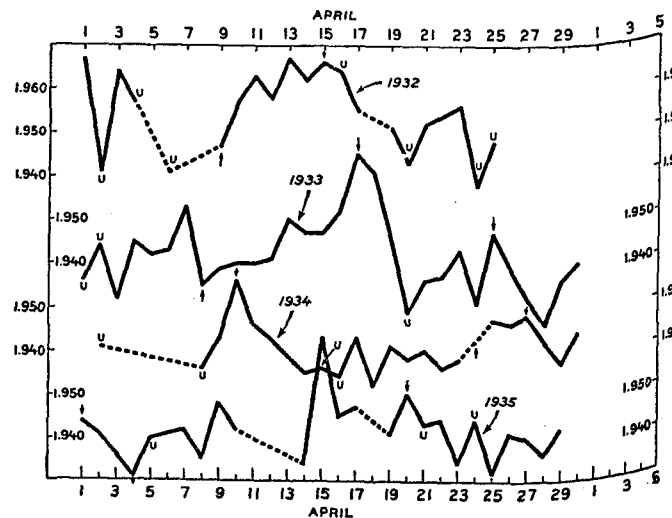
FIGURE 6.

ington temperature averages. Differences of one day would not be so important if a general trend of temperature were being considered, but since we are comparing patterns, a shift of one day introduces serious error.

Out of 33 cases given for April, errors have been found in dates assigned to 11. A similar analysis of August values shows 12 out of 30 to be in error.

Criterion 2 suggests that the effect of larger solar variations should be associated with larger temperature variations. In illustration of this, 2 cases of especially large rises and 3 of large decreases, in April were given. Taking, first, the large increases, their zero dates are April 4, 1926, and April 23, 1927. One of these begins with an almost unsatisfactory value and rises 23 points (where actually point is 0.001 g. cal/cm.²) in 2 days. The other decreases the first day to the preferred value for the 24th. No Montezuma value was given for this day. It is easy to find examples of more significant increases; for instance, beginning April 15, 1925, or April 9, 1926, which, at least, have satisfactory values at the terminal points of the increase.

The "exceptionally large" decreases begin on April 20, 1925, April 15, 1926, and April 30, 1926. The first is a decrease of only 13 points in 2 days; the second goes down

12 points in 2 days, from a $U+$ value and the third only declines 11 points in 1 day before it is interrupted by an increase.

Feeling that this important subject should be treated more extensively, I have made an attempt to compare Washington temperatures subsequent to large and small solar-constant changes, chosen objectively. The method of choosing zero dates was as follows: U or $U+$ values were not considered at either end of the change, but if they occurred as intermediate points, were given full weight. Large changes are those of at least 15 points in 2 or 3 days or 10 in 4 or more days. Small changes are at least 10 points in 1 day, 8 in 2 or 3 days, or 5 in 4 or more days. In determining these no interval was considered where the changes were not all in the same direction. As was the case in the paper considered above, actual departure of the values from the general mean was not taken into account.

There are two ways of testing the temperature departures. First, their distributions may be compared with those to be expected from chance sampling. For this purpose, I have plotted the average departures in terms of the standard errors of their means in figure 7. From purely random selection of dates, we may expect about 1 out of 20 averages to be greater than twice its standard error. It can readily be seen that this probability is not exceeded. Nor are variations generally greater after especially large solar changes.

A second test may be made by comparing the temperature fluctuations following rising and falling solar-constant values. It is obvious that there is little evidence of opposition. The correlations between temperatures following all ascending and descending values are $+0.10$ for April and -0.05 for August, both insignificant.

It might be said that a correlation coefficient does not express the entire similarity or difference between such series, since its calculation is based on departures from each series mean. If the mean temperatures after rising and falling solar constants differed, this would not be taken account of in the coefficient. Therefore, the differences between means were calculated, and are 0.13 and 0.49 degrees for April and August, respectively. The standard errors of these differences are 0.52 and 0.45, indicating that neither is significant.

It may be of interest at this point to see how the results compare for two separate periods, 1924–29 and 1930–35. In order to use as many cases as possible, large and small solar values for each case were combined, again, for April and August. The correlation coefficient between Washington temperature sequences following rising solar constants in April, for the two 6-year periods is

$r=-0.44$; for falling solar constants, $r=-0.02$; in August, increasing solar values, $r=+0.36$; decreasing, $r=+0.39$. Each of these was computed from 17 pairs— the zero day and 16 following days. The criterion that the average results should be similar, that is, that the correlations should be positive and significant, is definitely not fulfilled. Three of the coefficients seem rather high numerically. This is due, probably, to the serial correlation in daily temperature departures, which reduces the effective number of pairs in the calculation. Thus, in 17 days, there may be only, say, three or four

RATIO OF WASHINGTON AVERAGE TEMPERATURE
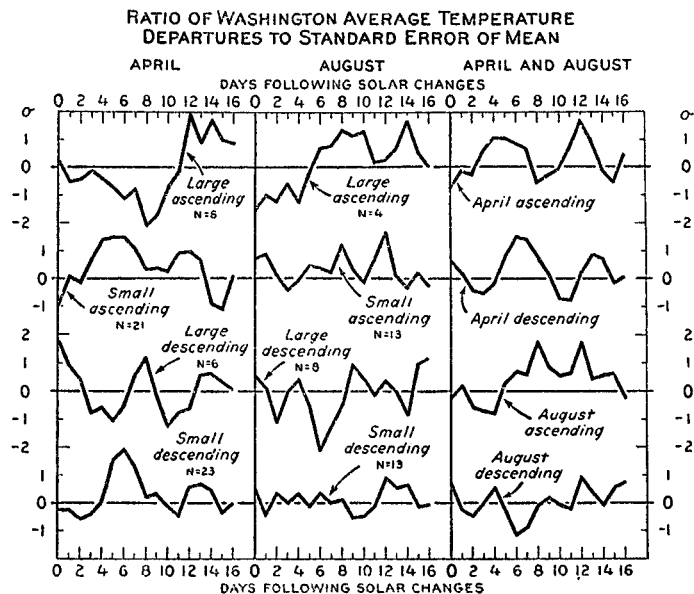DEPARTURES TO STANDARD ERROR OF MEAN

FIGURE 7.

observations which are really independent. It is not at all improbable that we should get by chance the high numerical values of the correlation which we observed.

Thus, the analysis shows that the data fail to fulfill any of the criteria set up. No doubt, if enough temperature records were examined, a few, purely by chance, would be found to support the hypothesis, but the evidence here indicates that there is no real relationship of the type investigated.

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REVIEW OF H. H. CLAYTON ON LONG-RANGE WEATHER CHANGES AND METHODS OF FORECASTING [1]

By Hurd C. Willett

Clayton summarizes his method of long-range weather forecasting in the following five points:

1. The analysis of weather phenomena into pulses, waves, or periods of different length; this is done for each station in a network of stations, and the latest values plotted on maps. From a succession of such maps the movements of areas of excess and deficiency can be followed and their future position anticipated.
2. Correlation of the meteorological pulses or waves with solar radiation pulses or waves found in the same way.
3. Projection of these waves ahead into the future, using for this purpose the mean length of the solar periods determined by experience and calculations.
4. Reading from each of the curves so projected ahead the value of some particular time or epoch desired, and summing the different values thus obtained.
5. The process described in (3) is done for a network of stations, the sums are plotted on maps and lines of equal value drawn. These maps then become a forecast for the area covered.

1. THE ANALYSIS OF WEATHER PHENOMENA INTO PERIODS

In the smoothing of his data and their analysis into periods of different length, Clayton has made use principally of the methods of moving averages and of harmonic smoothing. He has experimented with a number of procedures in harmonic smoothing and with weighted and unweighted moving averages involving a wide range in the terms used. He has come to the conclusion that the harmonic smoothing methods are best suited to the data with which he is working, so that he has come to depend upon them almost exclusively. He points out, however, that for the irregular type of meteorological period which he is investigating it is impossible to use mean values for each term of the period (i. e., means of the terms at the same points in a number of periods to give an average period), but that it is necessary to compute sine and cosine functions for each individual period separately so that its variations can be followed. He says "The type of harmonic formula used for this purpose is as follows: Let $l_0, l_1, l_2 \ldots l_{n-1}$ be observed values which are associated with equidistant values of some argument, say time; then the single periodic terms, namely, coefficients of a sine curve drawn through the observations, may be represented by the trigonometric formula:

$$L = A_0 + A_1 \cos \phi + B_1 \sin \phi$$

in which

$$A_0 = \frac{\Sigma l}{n}$$

$$A_1 = \frac{\Sigma l \cos \phi}{1/2 \; n}$$

$$B_1 = \frac{\Sigma l \sin \phi}{1/2 \; n}$$

$$\frac{A_1}{B_1} = \tan \theta$$

$$a = \sqrt{A_1^2 + B_1^2} = \frac{A_1}{\sin \theta}$$

$$\phi = \frac{360°}{n}$$

and $\theta =$ the angle of the epoch, namely, the angular distance from 0 to the part of the sine curve at the beginning of the period, while a is the amplitude and n the number of terms used."

In his analysis of weather phenomena into periods, Clayton works with the departures from normal of the pressure, temperature, and rainfall, in order to eliminate the annual period. He has found numerous periods in such data, ranging from only a few days to a good many years in length. In the smoothing process the number of terms are chosen to show up best the particular period whose presence he suspects or wishes to bring out.

The question may be raised as to what extent the weighting of the middle terms which is implicit in Clayton's harmonic smoothing method may impose upon the data the very periods he wishes to establish. In this connection, L. F. Page states:

The positive and negative weighting does, in fact, produce apparent periodicities of a length equal to or slightly shorter than the number of terms involved. They are subject to "change of phase" without regard to the nature of the original series. To illustrate the effect of Clayton's method, I took 4 series of random numbers, using Tippetts' tables as a code for normal distribution, but entirely unrelated as to sequence. I treated each of these with Clayton's 12-term method. The results in graphical form are shown in figure 1. It is interesting to note the agreement in B and D, even in the long interval where a "change of phase" occurs. These were not picked as the best of a large number, but were the only ones tried. In view of this, his apparent agreement between solar radiation values and pressures (fig. 2, below) seems to me to be far short of convincing. Incidentally, he probably had an added factor to increase the smoothness of his curves in the serial correlation in monthly values.

Clayton considers the variations of pressure to be basic among the meteorological elements. He finds that variations in temperature and to some extent also precipitation are fairly well correlated with those of pressure. The correspondence between the behavior of the different following meteorological elements is brought out in the following way: The records of the departure from normal of time for pressure are analyzed over an extended period of time for each one of a number of stations in a given area. As soon as a period of oscillation has been established for the given area the value of the departure from normal of the pressure at each station at any arbitrarily chosen point of time may be plotted on a map. The point of time chosen will normally be one of a maximum or minimum of the oscillation of the pressure departures. When lines of equal positive or negative departure have been drawn on the map, centers of maximum positive or

[2] In commenting on this paper, Mr. Clayton objected to the implication that random series, smoothed by his method, could be really correlated. He is correct in saying that there is no true correlation between the parent populations of random series each treated by his method. There is, however, a greater probability of getting a correlation of a given size in the smoothed data than in a sample of the same size in the original random series. It may be pointed out that this difference in probability is the essence of the serial correlation problem in time series.

Since this report was written, Dr. T. E. Sterne of the Harvard Astronomical Observatory obtained very similar entirely independent results, using dice throws to obtain random series. He found the same tendency toward agreement with "changing phase" after smoothing by the harmonic method.—L. F. P.

[1] Monthly Weather Review, vol. 64, p. 359 (1936).

negative departure may be located. Clayton calls the areas of positive pressure departure baropleions and those of negative departure baromeions. When they are determined for temperature in the same way they are referred to as thermopleions, and for precipitation they are called ombropleions. If such pleions and meions are plotted for the maximum and minimum points in the pressure periods, then they show the complete oscillation of the pressure departure from normal for the given period. The thermopleions and ombropleions may be prepared for the same periods and the location and intensities of the pleions and meions compared with the corresponding baropleions and meions. In this way the correspondence in the periodic behavior of the different meteorological elements is established, and on this correspondence may depend the forecasting of the elements of temperature and precipitation.

However, as might be expected, the correspondence between the baropleions and the thermopleions and ombropleions is not at all rigid. In the United States Clayton finds that in general areas of positive pressure departure are associated with areas of negative departure of temperature and precipitation from the normal, and vice versa. But the respective pleions are by no means
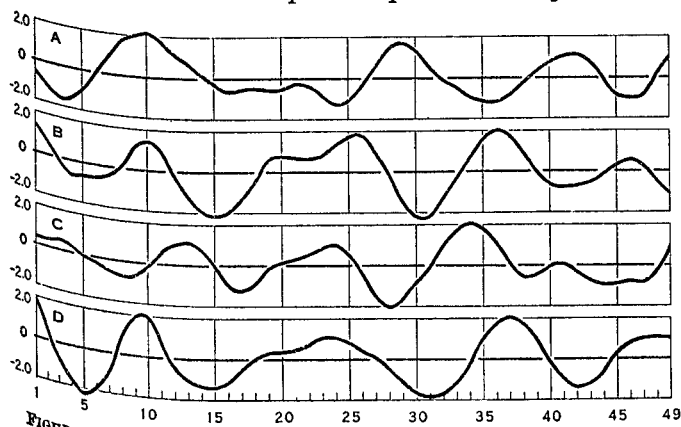

FIGURE 1.—Three-term running mean of 12-term harmonic smoothing of random numbers, four separate series.

coincident, nor is there any very close correspondence in their intensities. Consequently, although in a general way the correct forecasting of baropleions may be used to indicate the expected sign of the departures from normal of temperature and precipitation, the forecasting of the magnitude of such departures is open to question and even the sign of the departure will be uncertain in all boundary regions. Thus it appears that even if the basic baropleions and meions represent real oscillations in the data and can be reliably predicted, their prediction still leaves a good deal to be desired as a basis for the forecasting of the departures from normal of temperature and precipitation. As will be pointed out under (5) below, Clayton now prefers to compute the future baro-, thermo-, and ombro- pleions and meions independently and then check them one against the other.

But as Clayton points out, the pressure oscillations about the normal which are found by analysis are by no means of a regularity which permits an accurate projection into the future. The pressure periods on which the baropleions and meions depend are found to be somewhat irregular both in length and amplitude, and occasionally they undergo a complete reversal of phase. But referring again to Page's remarks, we are reminded that there is some question as to whether these periods have any reality at all, or are only imposed by Clayton's smooth-

ing method. If this is accepted as the complete explanation of the periods found by Clayton, then of course the meions and pleions lose all forecasting significance. As Page states:

This probably explains why the centers of action [meions and pleions] move irregularly * * * they have no physical basis, but are the chance reactions of geographically correlated series responding to harmonic smoothing in much the same way as random numbers would. The geographical correlation explains the gradual change in phase from one region to another.

i. e., the fact that a well-ordered system of meions and pleions is obtained. The question of the reality of Clayton's periods cannot be determined finally without further statistical investigation, but the fact must not be lost sight of that the entire statistical basis for his forecast method is open to serious question. Because of the irregularity in the behavior of his baropleion and baromeion systems Clayton was convinced of the necessity of finding some controlling factor which would explain these pressure oscillations sufficiently to serve as a basis for forecasting them. It was in this connection that he became interested in the study of the variations of the
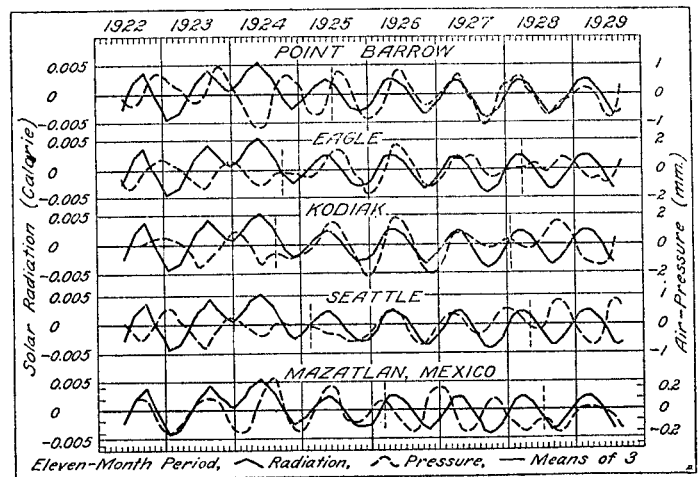

FIGURE 2.—Eleven-month period in solar radiation and pressure.

solar constant which he found to have a close connection with the meteorological periodicities.

2. THE CORRELATION OF METEOROLOGICAL AND SOLAR RADIATION PERIODS

Clayton has investigated at length, by various methods of analysis, principally the harmonic, the occurrence of periodicities in the solar constant and sunspot activity. As a result of much experiment he has concluded that the double sunspot period of 22.5 years is the most successful basic period of analysis for solar data, more successful than the 11-year sunspot period. Fractions of the former period, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{22}$, etc, can be used successfully in the analysis of solar data. An 11-month period ($\frac{1}{24}$) is found to be especially well pronounced, while periods even as short as 10 days are found to be useful. Of course, his statistical smoothing method is open to the same criticism in the case of the solar data as in that of the meteorological data.

Clayton finds a correspondence between oscillations about the normal of the meteorological elements, especially pressure, and the principal periodic variations of the solar constant. The reality of this correspondence remains open to doubt. (See Page's remarks.) However, this correspondence is by no means direct and simple. In

some localities the apparent baropleions are found to be exactly in phase with the solar constant period, in other localities the two sets of oscillations are found to be just opposite in phase, and in intermediate regions inter-

difference between pressure departure oscillations and those of the solar constant seems to correspond in the first place to an immediate reaction in the earth's atmosphere at low latitudes to the changes in the solar con-
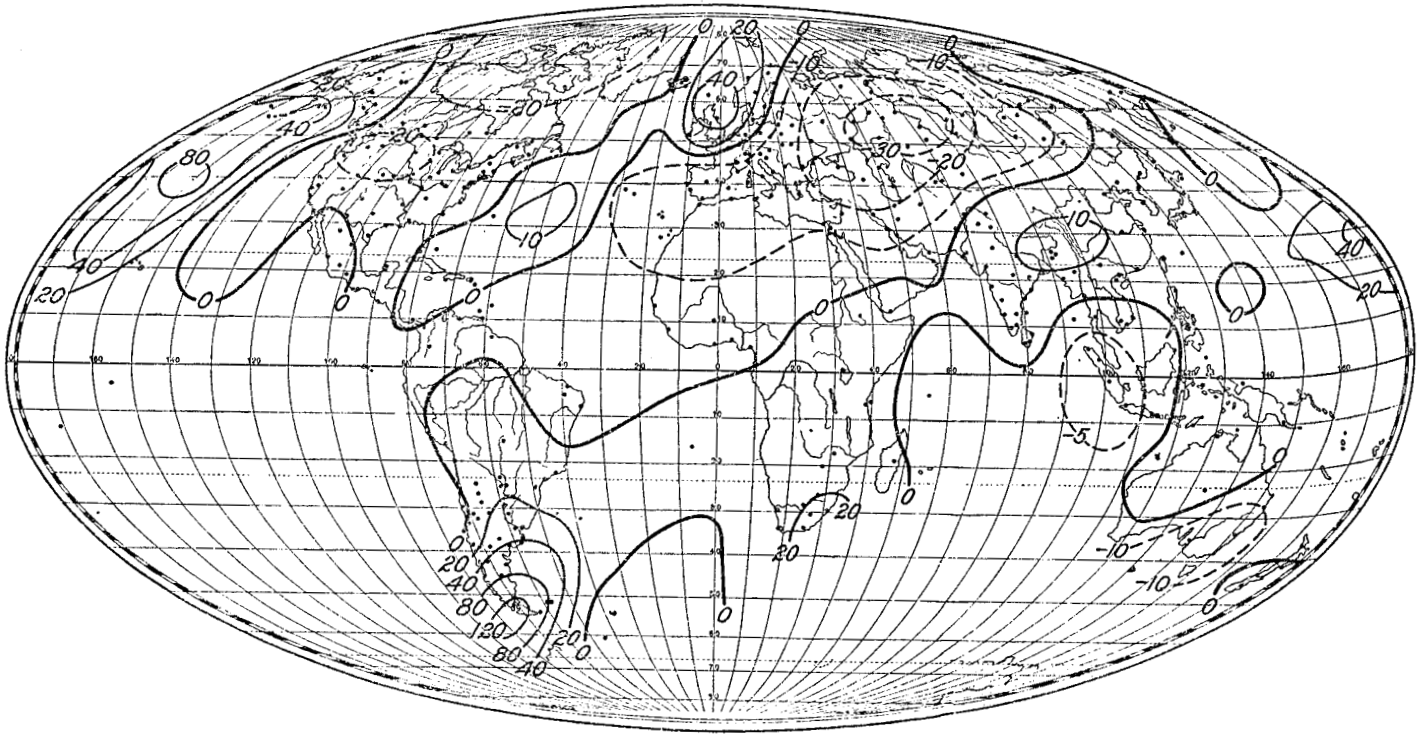

FIGURE 3.—Departure of pressure at time of maximum of solar radiation in 11-month period, in units of 0.01 millimeter.

FIGURE 4.—Departure of pressure at time of minimum of solar radiation in 11-month period, in units of 0.01 millimeter.

mediate relationships are found. Furthermore the phase relationship usually shows a tendency to progressive change at one and the same station. Such changes are especially well pronounced in the case of the 11-month cycle Clayton shows, in a general way, that this phase

stant, a reaction which is gradually propagated to and hence appears delayed in phase at higher latitudes, and in the second place to the opposite sense of this reaction both thermally and barometrically over continental and over oceanic areas. In other words, what appears to be the

effect of changes in the solar constant is a slight change in the positions or relative intensity of the centers of action of the general circulation which are conditioned by maritime and by continental influences. These effects appear first at low latitudes and move slowly northward. The amplitudes of the pressure oscillations, or the magnitude of the baropleions and meions with which Clayton is dealing in this particular discussion, appear at first observation to be very small to be made the basis of any forecast of the principal departures from normal of temperature and precipitation. The intensity of the baropleions and meions which he discussed in the 11-month period and which as a rule cover the greater part of an ocean or continent in their horizontal extent is in general only 1 millimeter or less. (See figs. 3 and 4.)

3. THE PROJECTION OF SOLAR AND METEOROLOGICAL PERIODS INTO THE FUTURE

The advantage of correlating the oscillations about the normal of the meteorological elements directly with the variations of the solar constant lies in the fact that, since the solar constant variations are more regular and predictable than the meteorological, insofar as any relationship can be established between the two, the more predictable solar constant variations can be made a basis for the extrapolation of the meteorological periods into the future. Clayton's preferred method of procedure at present is about as follows: In the first place a long series of the variations of the solar constant are smoothed up to the latest available data. The smoothing is carried out with different numbers of terms to bring out all the assumed principal periods in this quantity, although it remains to be proved whether these periods are "brought out" or "produced" by the analysis. Then the departures from normal of the meteorological elements, pressure, temperature and rainfall, are taken at a definite station for the same time interval as the solar constant values. The departures of each of these elements are taken separately and smoothed for each of the periods for which the solar constant data were smoothed. Then each smoothed curve of each meteorological element is superposed on the smoothed curve of corresponding period of the solar constant. In this way the periods of each of the meteorological elements at a given station can be directly compared with the corresponding periods of the solar constant so that the correspondence between the two is represented graphically. Since each of the solar constant periods can be projected with a fair degree of regularity one or two periods into the future it is possible to extrapolate each of the period curves of each of the meteorological elements a corresponding distance into the future by assuming that the relationship to the corresponding solar curve undergoes no marked change. Clayton points out that in general this assumption will hold for one or two periods, but occasionally there appears a reversal of phase in the relationship between the two curves so that the extrapolated meteorological curve becomes quite erroneous. However, his forecast procedure calls for the extrapolation of each of the smoothed curves of the departures from normal of each of the meteorological elements. But it is evident that there must be many cases in which this extrapolation becomes pretty much a matter of imagination, cases which undoubtedly must frequently occur when the relationship between the smoothed curve of the meteorological data and the corresponding smoothed curve of the solar constant data must be rather nebulous, to say the least. There are many cases, however, where the correspondence

between the two sets of curves appears to be surprisingly good. I express this skepticism of the consistency of the agreement of the meteorological curves with the solar curves in the first place because the entire statistical basis of his period determination is open to question, in the second place because even if the periods are found to be real it is difficult for me to believe that variation of the solar constant constitutes the only important factor which determines the departures from normal represented by even the longer period meteorological oscillations, and in the third place because if even this also were true, the effect in many cases would doubtless be so involved and indirect that a direct comparison of the two sets of curves inevitably must be indecisive and confusing. Meteorological relationships are always complex. It is to be assumed that the curves chosen by Clayton for illustration of this agreement are some of the best which he has obtained rather than any of the poorer ones. (See fig. 2.)

4. THE SUMMING OF THE PROJECTED VALUES OF THE DEPARTURE FROM NORMAL OF EACH METEOROLOGICAL ELEMENT AT A GIVEN TIME OR EPOCH

Evidently the next step in the preparation of the forecast of the future trend of each of the meteorological elements at the chosen station must be the summing up of the departure from normal as given by the extrapolation of each of the different smoothed curves. That is, the value of the departure from normal of the pressure, temperature, or precipitation at the given station at any given instant of time in the future is obtained by adding the departures from normal indicated for that particular point in time on each of the extrapolated curves for the particular element in question. It is evident that a forecast prepared in this way will have a real basis only insofar as (a) the basic periods found by the smoothing of the data are real, (b) the projection into the future of each of the different periods into which the variations of the solar constant can be analyzed is justified, and (c) the oscillations in the departures from normal of the individual meteorological elements are definitely correlated with the different periods into which the solar constant variations are analyzed. The uncertainty of the first of these assumptions has been indicated above. The second of these assumptions is reasonably justified if the first one holds, but the third assumption would appear rather weak for the reasons given at the end of the last paragraph. The final justification of the whole method must depend upon the extent to which the forecasts verify.

5. THE PREPARATION OF MAPS FOR REGIONAL FORECASTS

The process just described for a single station can be carried out for any number of stations. The analysis of the solar constant data need be carried out only once, but the analysis of the pressure, temperature, and precipitation departures into the corresponding periods must be carried out separately for each station. In this manner, the departure from normal of each of the meteorological elements at a given instant of time or at a given point in a period of oscillation may be computed for as many stations as desired and over as large an area as desired. If these synchronous values of the departures from normal of each of the meteorological elements are plotted on maps, lines of equal departure may then be drawn, and so a regional forecast map of the area concerned has been prepared. Such a map will represent the pleions and meions of the different meteorological elements for a given day,

week, month, season, or year in the future, depending upon the time unit in which the departures from normal of the observational data were expressed. When the baropleions, thermopleions, and ombropleions have been determined independently in this manner, it is advantageous to compare them one with another and to judge them critically from the point of view of their mutual consistency as determined by experience. It was pointed out at the beginning of this discussion that the baropleions are considered by Clayton to be more basic than the others in this method of forecasting. If the pleions and meions of the three elements are in reasonable agreement one with another in the manner in which experience has shown them to be related, then the general forecast can be considered as having been checked and double checked, but if they are in marked disagreement, the forecast must be considered as being less satisfactory. In that case the logical procedure must be to modify the indicated future thermopleions and ombropleions to bring them into better agreement with the baropleions.

THE VERIFICATION OF CLAYTON'S FORECASTS

As is inevitable in the present state of long-range weather forecasting, the verification of Clayton's long-range forecasts is far from satisfactory. The justification of his whole system, however, must depend on whether the verification of his forecasts is better than might be expected by pure chance. The following verifications were made by Page, using five quarterly forecasts in 1936 and 1937. Unfortunately shorter range forecasts made by Clayton were not available for verification. Page's explanation follows:

State averages weighted by areas were used where forecasts were made for complete calendar months. Where months were divided temperature verifications were based on all first order stations in the region and rainfall was computed from all stations, including cooperative reporters.

Most of the forecasts were for either above or below normal, so

that a 50 percent probability might be expected by chance. Of these 75, where no uncertainty of interpretation could occur, there were 38, or 50.6 percent correct. Of the 11 other temperature and precipitation forecasts, 5 may be considered to have a 50 percent probability by chance, 2 have 33 percent, 1 other has somewhat less than 50 percent and 3 have considerably above 50 percent chance of being right, so there is probably no appreciable error in assuming an average of 50 percent probability for all 86 forecasts. The results are:

	Number of forecasts	Number correct	Percent correct
Temperature	61	32	52.5
Precipitation	25	14	56.0
Total	86	46	53.5

Other forecasts are as follows:

A. *Cold waves.*—These included 1, 2, or 3 consecutive days. The probability on the basis of the 1936–37 winter for which the forecasts were made was near 50 percent that a storm would be somewhere in the region in any 2-day interval. The results showed 6 right out of 11.[3]

B. *Frosts.*—These are very general and the probability varies. For instance, a "general" frost was forecast in the North Atlantic and East Lake Region by the middle of October. This is very likely, whereas the forecast of "possible" frosts in the Cotton Belt in late September or early October has a much smaller probability of coming right.

C. *River conditions.*—Here again the probabilities cannot be computed precisely. Two out of three were correct, but one of these was that there would not be enough rain in September and October 1936 to restore the water level and cause floods. By late August when the forecast was made, the chances of success were quite high.

Considered as a whole, it is not possible to say that this group of forecasts gave any better results than could be obtained by chance.

From this discussion it would appear that as far as concerned, practical long-range weather forecasting is concerned, Clayton's method has little or nothing of direct value to offer in its present form.

[3] The verification of cold waves may well be made more rigid, but in that case both the percentage of verification and the probability will be decreased proportionately.

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